Essays in Empirical Corporate Finance

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Table of Contents

List of Figures

Agency versus Hold-up:

On the Impact of Binding Say-on-Pay on Shareholder Value

Evidence of Excess Comovement in US Mergers

Default Risk and Bondholder Wealth in US Mergers

List of Tables

Agency versus Hold-up:

On the Impact of Binding Say-on-Pay on Shareholder Value

Evidence of Excess Comovement in US Mergers

Default Risk and Bondholder Wealth in US Mergers

Part I: Introduction

1. Introduction

Empirical work is an important pillar of academic research in finance. On the one hand, it provides a way to verify and test the validity of theoretical models while, on the other hand, it informs theory of novel developments and changes observed in the markets. As such, the empirical work is in constant exchange with the theoretical progress.

The work of the empirical guild is to a large part driven by the availability of data, as many theoretical predictions are only testable if the appropriate data are being collected and made available. The technological progress in recent decades alleviated the collection of data and, equally important, provided empirical researchers with tools to work with ever growing datasets. This mounting availability of more and new data is therefore also a chance for empirical researchers to reconsider earlier, well-appreciated theoretical work. At the same time, empiricists have put considerable effort into advancing the applied methodology to thoroughly verify causalities predicted by the theoretical models. The increasing methodological rigor as well as the growing universe of data have allowed the empirical research to steadily progress and provide valuable insights to policy makers as well as practitioners.

This thesis presents three empirical research papers that have a firm's shareholders as a common denominator. Shareholders play an important role in the economy. They provide funds for firms and take a share of the business risk upon them. By the second quarter of 2012, the value of all equities traded on the New York Stock Exchange (NYSE) amounted to USD 13.3 trillion.[1] This compares to a total of debt outstanding by firms in the US of USD 8.47 trillion in the same period.[2] The important position of the shareholder within the firm confronts him with various conflicts of interest that involve other stakeholders. The most important frictions concerning shareholders have been outlined in the seminal work of Jensen and Meckling (1976).

[1] Reported by the World Federation of Exchanges.

[2] Reported by the Securities Industry and Financial Market Association (SIFMA).

The most widely discussed conflict in this context is certainly the agency problem between shareholders and managers. In general, shareholders of firms that become big enough to be listed on a stock market delegate the operative management to hired managers and set up a board of directors as a supervisory authority. In this setting, shareholders will commonly limit the exertion of their power to the votes at the annual meeting of the firm. While this structure has generally worked well in the past, there have always been cases where the system of checks and balances between shareholder, management and the board has broken down, or at least been impaired (e.g. Enron, Lehman Brothers, etc.). The catalyst was, more often than not, related to compensation packages (and thus incentives) granted to management and endorsed by the board. The public discontent over some of these pay practices has become large enough to attract the attention of policy makers eventually. As a result, they have started to design laws that shift more power towards shareholders. The analysis and development of such regulation is a very topical and ongoing endeavor.

Shareholders are the ultimate owner of the firm and as such generally coin the firm according to their visions and believes. While they usually delegate the operative control, they retain the power to decide over the most influential resolutions for the firm going forward (for example, the approval of large mergers and acquisitions). At the same time, they have the control to assign board seats and thus broadly influence the future of the firm in terms of corporate culture and risk taking. Hence, the impact that shareholders have on the risk of a firm relative to the market, as measured for example by the equity beta, is a controversial one in the literature. While theoretical work, mostly in the field of asset pricing, is critical of such an impact, the empirical analysis has come up with some evidence in favor of this idea.

However, shareholders not only determine their own risk, but are also responsible for how other important stakeholders in the firm, for example bondholders, fare. Contrary to shareholders, bondholders provide capital to the firm without having any direct say, but with a risk that is markedly lower than that of shareholders. The different liabilities of

the investor attached to these two kind of financing give rise to the debt agency problem. Because shareholders have a limited downside potential in their investment (i.e. the share price can only drop to zero), but an unlimited upside potential, they generally are in favor of increasing the risk of a firm (risk shifting). Bondholders, on the other hand, commit their money to the firm over the medium to long term and, in the simplest case, set a bond's future payoff profile at issuance. Any increase in risk thereafter, which is generally supported by shareholders, does impair the value of the bondholder's investment. While the theoretical framework for this agency has been established, the empirical evidence considering debt is still limited.

The goal of the academic research on the firm, on its owners and on other stakeholders is to understand where problems that distort an efficient functioning of this structure may occur and how they can be solved or at least be moderated. Findings of this research inform policy makers as well as investors. This thesis takes a closer look at the three above mentioned domains of shareholders and intends to contribute to the current level of empirical research by providing a rigorous methodological and data-specific analysis. I am very grateful to Per Östberg and Alexander Wagner for their great guidance and support as well as their critical and helpful comments which were of great value to advance my own thinking as well as all three research papers. During the process of developing ideas and writing, I have also profited, and am very thankful, from valuable discussions and remarks by, among others, Kjell Nyborg and Rüdiger Fahlenbrach.

2. Summary of Research Results

This thesis consists of three separate research papers. In this section, I briefly summarize the projects and provide a short summary of the findings.

The first research paper, titled *Agency versus Hold-up: On the Impact of Binding Say-on-Pay on Shareholder Value*, is joint work with Alexander Wagner. We consider the relation

between shareholder power and shareholder value by analyzing a particular event in the Swiss legislative process, namely the successful completion of a public referendum known as "Anti-Rip-Off-Initiative" (*"Initiative gegen die Abzockerei"*). This particular initiative concerns all listed companies domiciled in Switzerland and contains different provision that would considerably strengthen the power of shareholders, especially on the issue of management compensation. Specifically, the key demand of the initiative is a compulsory annual *binding* vote on total compensation (sum of fixed and variable pay). The board of directors proposes a compensation package for each body, which then needs to be approved by the general assembly for the year to come. Anything that the board wishes to pay on top of that, during or after the year, for example as a result of an extraordinary change in the business environment during the year, has to be endorsed by the general assembly ex post.

Overall, for the 100 largest firms listed in Switzerland, the event led to a negative and significant abnormal announcement return of 1.88% over a three day window. This compares to an insignificant reaction of -0.03% for an industry and size matched sample of German firms which were unaffected by the initiative. Further analysis was conducted by considering the cross-sectional variation in reaction to this event. The particular design of the initative, together with the fact that it is one of the rare *binding* say-on-pay provisions, leads to two hypotheses.

First, the distinctive way in which shareholders gain additional power could *hold up* firm specific investments by other stakeholders, in this case management, because they expect to not receive the full return on any extra effort provided. Consistent with this hold-up hypothesis, we find that firms where specific investments by the CEO are particularly important, for example firms with younger CEOs, with CEO's of a shorter tenure or in industries where contracting is more difficult because of higher cash flow uncertainty, reacted more negatively to the event. This finding is consistent with predictions from earlier theoretical literature by Burkart, Gromb, and Panunzi (1997). To our best knowledge, this is the first paper to provide empirical evidence that increased shareholder power can lead to a hold-up problem

and thus be costly for shareholders. This cost is an important issue, especially in the process of crafting regulatory rules on shareholder power.

Second, the increase in shareholder power is widely conceived as improving the *alignment* between shareholders and management. This is for example shown in work by Becker, Bergstresser, and Subramanian (2012) and Cohn, Gillan, and Hartzell (2011), who find that developments towards increased proxy access for shareholders resulted in positive stock price reactions for firms where shareholders are more likely to take advantage of that access. The reduction in agency costs through better alignment should especially add value to firms that are currently poorly aligned. This conjecture also finds support in our data as, for example, firms that have underperformed size- and risk-adjusted benchmarks in the past, fare relatively better in terms of abnormal returns than outperforming companies. A closely related research paper by Cai and Walkling (2011) assesses the reaction to a law provision in the US, requiring *advisory* say-on-pay. They find neutral to slightly positive reactions for firms that currently have a poor alignment of shareholder and management interests.

This research extends the current literature by highlighting an important trade-off between hold-up costs and alignment benefits when shareholder power is increased. The findings inform the current policy discussion on how to design shareholder rights laws.

Evidence of Excess Comovement in US Mergers is the second research paper which is joint work with Per Östberg. In this paper, we provide evidence of stock market segmentation by studying US mergers. According to the classical asset pricing theory, the return required by investors in a frictionless market depends on the comovement of a firm's assets with the market only. However, Froot and Dabora (1999) and Chan, Hameed, and Ting Lau (2003) provide empirical evidence that in an international context, frictions exist and lead to market segmentation. They show that the comovement with the market changes significantly if firms change their listing location.

A significant change in comovement as a result of cross-boarder mergers is found by Brealey, Cooper, and Kaplanis (2010). They show that the comovement with the exchange

of the acquiring firm increases in cross-border mergers. Our paper adds to this literature by documenting the existence of *within-border* segmentation. Moreover, Pirinsky and Wang (2006) document that when firms move their headquarters, they increase their comovement with an index of local firms. In contrast, we use mergers to document the existence of *within* index segmentation as opposed to *across* index segmentation.

To test for market segmentation, we use mergers as a means of identification. Target shareholders who tender their stake in the merger will no longer be part of the merged firm's shareholder base and hence not contribute to the merged firm's comovement anymore. If comovement is determined not only by a firm's assets, but also by its shareholders, we should observe a shift in the comovement of the merged towards the acquiring firm. The segmentation hypothesis predicts that the shift towards the acquirer's comovement is greater when more target shareholders exit. Therefore, cash mergers, where shareholder exit is particularly pronounced, should result in especially large shifts in comovement.

We measure this shift towards the acquirer, the excess comovement, by comparing the equity beta of an expected ex ante market-value weighted merged firm with the observed equity beta of the merged firm ex post.

In the overall sample, we find that, when the ex-ante acquirer beta is higher than the target beta, the comovement of the post-merger firm is 8.26 percent higher than the ex-ante expected comovement. For deals that are paid in cash only, and hence lead to the largest exit of target shareholders, the ex-post comovement is 20.65 percent higher than the ex-ante expectation. In contrast, stock mergers, which will not lead to a direct exit of target shareholders, show no effect at all. We find similar results for sample splits along other characteristics that are likely to reveal a variation in the change of the shareholder base. For example, if the merger leads to the inclusion of the target in the S&P 500, and hence part of a new investor's habitat, we find a greater shift towards the acquirer's comovement.

The evidence provided in this paper is aimed to inform researchers and practitioners alike of an important market friction that has so far not been documented on the essential

national, within-index level. The evidence that segmentation is to a certain extent a result of a firm's investor base has important implications for the assessment of risk in terms of beta. Particular consideration has to be given to the analysis of situations in which a large share of the investor base changes.

The third project, *Default Risk and Bondholder Wealth in US Mergers* takes yet another look at how shareholders influence other stakeholders within a firm. In particular, it documents the effect of a merger announcement, which ultimately has to be approved by shareholders, on the default risk of debt. The latter is measured by credit default swap (CDS) spreads. The close link between CDS spreads and bond value allows, furthermore, to not only consider abnormal changes in default risk to the announcement, but also to put a price tag, in terms of value created or destroyed, on the deals.

Previous literature in this field has provided ambiguous results on how merger announcements effect debtholders. The most comprehensive study on this topic is by Billett, King, and Mauer (2004). They find that bond prices of target firms react positively and significantly to merger announcements. This suggests a reduction in risk. Acquiring bonds, on the other hand, show a negative price reaction which implies an increase in risk that is, however, very small. The issue with this strand of literature is the comparably low trading frequency and the relatively low degree of standardization of bonds. This impairs the power of the usual test statistics considerably, as it is shown, for example, by Bessembinder, Kahle, Maxwell, and Xu (2009). The use of CDS spreads increases the power of the statistical analysis, as trading is more frequent in this instrument. Moreover, the close link between CDS spreads and debt value further allows to measure the economic effect of the merger announcement. This is in contrast to related work by Furfine and Rosen (2011) who document an increase in the probability to default within the next year by, on average, 93.6 percent as a result of a merger. They measure changes in default with the KMV default score and thereby overcome the standardization issue of the previous literature, yet cannot comment on the economic impact of their findings.

In my sample of merger announcements by US acquirer's, I find that the acquirer's debt default risk increases in two thirds of the considered deals. The average cumulated abnormal spread change in the three day window around the announcement is +4.95 percent for acquiring firms. This is significantly different from the model prediction of a non-positive effect. I therefore analyze the cross-sectional variation in abnormal reactions to the announcement. Building upon a widely used asset pricing framework by Merton (1974) and linking it to a CDS pricing model, I derive predictions on the changes in default risk.

In the data I find evidence that the abnormal increase in CDS spreads is particularly large and significant for mergers that are done within the same industry, where cash flow diversification possibilities are comparably small. In contrast, transactions across industries, which can profit from this cash flow co-insurance effect, only show an insignificant change in abnormal CDS spreads.

The size of the insurance provided by CDS in single firms also helps to explain the abnormal change in default risk. Mergers in which the outstanding CDS cover an above median share of the acquirer's debt outstanding increase default risk significantly more on average. These results suggest that 'empty creditors', i.e. bondholders that are insured against default, seem to also matter in the context of mergers.

Moreover, the risk does not seem to be driven by the ex-ante differences between target and acquirer characteristics, but rather by how a deal is structured, i.e. by how acquirer characteristics change as a result of the merger. For example, it is not the fact that the target has a higher leverage ratio than the acquirer that drives the increase in CDS spreads, but rather the circumstance that the deal leads to an increase in the acquirer's level of leverage.

Interestingly, the impact of mergers on bondholder wealth is rather moderate, with an average loss of USD 17.3 million. What is striking, however, is the comparable change in wealth of current CDS investors. On average, outstanding CDS contracts gain USD 656 million in gross value as a result of the merger announcement.

This paper provides novel evidence on how the default risk of debt changes as a result of

a merger announcement and what this means in terms of wealth for debt and CDS investors. It extends the relatively scarce and ambiguous academic knowledge in this particular field and informs practitioners, especially bond investors, on a potentially conflicting issue.

Summing up, I hope that the results presented in this thesis help to extend the current research frontier and foster our understanding in the three domains of shareholders I pointed out. Equally important, in my opinion, is the ambition to contribute not only to research per se, but also inform and be of added value to practitioners and regulators.

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Part II: Research Papers

Agency versus Hold-up: On the Impact of Binding Say-on-Pay on Shareholder Value*

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Abstract

A policy experiment in Switzerland sheds light on the hitherto theoretical concept that shareholders may prefer to have limits on their own power. The empirical evidence suggests a trade-off: On the one hand, binding say-on-pay provides shareholders with an enhanced ability to ensure alignment. On the other hand, the prospective law under consideration would give shareholders an ability to partially set pay ex post which may distort ex ante managerial incentives for extra-contractual, firm-specific investments. Thus, shareholder power reduces agency costs, but accentuates hold-up problems. These findings inform the design of policy.

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1. Introduction

In this paper, we assess the stock market reaction to the unexpected announcement of a prospective change in Swiss law that would considerably increase the power of shareholders by subjecting executive and board compensation to a *binding* shareholder vote. 70% of Swiss public corporations responded with negative abnormal stock returns to this natural experiment, an at first surprising result: Many shareholders seem to dislike the additional power they would obtain. Closer inspection reveals that the cross-sectional variation of shareholders reactions reflects the benefits and costs of binding say-on-pay in their respective companies. While the widely discussed alignment benefits can explain part of the stock price reactions, we provide novel evidence that shareholders also worry significantly about the distortion of executives' extra-contractual incentives that comes, as we show, with the design features of the initiative. Our central finding is, therefore, that shareholders tend to face a trade-off between agency and hold-up when it comes to the role of shareholder power in shareholder value creation. Regulation needs to take this trade-off into account.

Understanding the advantages and disadvantages of shareholder power is not only a long-standing academic question, but also an issue of significant policy relevance. Several recent proposals in the U.S. and elsewhere consider enhancing the power of shareholders. Among these, the question of how (and if at all) to design say-on-pay regulation is particularly topical. In the U.S., a first proposal by Representative Barney Frank to provide shareholders with an advisory vote on executive compensation passed the House in 2007. While it was never picked up by the Senate, a similar proposal later became part of the "Dodd-Frank Wall Street Reform and Consumer Protection Act" of 2010. As a result thereof, the SEC adopted a rule in January 2011 that requires an *advisory* shareholder vote on executive compensation at least once every three years. However, proposals for binding say-on-pay rules have also been brought forward, and proposals to further strengthen shareholder power are likely to

keep appearing.[1] In Europe, the European Commission has been issuing recommendations in connection with directors' remuneration ever since 2004 (see European Commission (2010) for a review). In December 2012, the European Commission (2012) announced that it will propose a guideline for say-on-pay in member countries in 2013.[2] A large number of countries is considering or has implemented a (partially) binding say-on-pay rule.[3]

A recent natural experiment in Switzerland provides an opportunity to investigate how shareholders perceive the benefits and costs of a particular law, designed to provide them with a binding say-on-pay. Specifically, on February 26, 2008, it became public that more than 100'000 Swiss voters had signed the "Anti-Rip-Off-Initiative" ("Fat-Cat-Initiative," *"Initiative gegen die Abzockerei"*), a law proposal whose central element is the introduction of binding say-on-pay for shareholders of all publicly traded firms. This meant that the proposed bill was set for a popular vote with obligatory adjustments to the Swiss constitution in case of a positive outcome.

To obtain insights into the channels through which an increase in shareholder power may transmit to shareholder value, we investigate cross-sectional differences in the reactions of firms, linking them to the extent to which firms benefit from (or are hurt by) binding say-on-pay. We test two main hypotheses in this respect.

First, our primary focus is on a channel that has, for lack of appropriate data and settings, received little empirical attention so far, but that has long been proposed in the theoretical literature on optimal shareholder rights and managerial discretion (see in particular Burkart, Gromb, and Panunzi (1997); Blair and Stout (1999), and Stout (2003)): When shareholders have more power, other stakeholders who make specific investments in the firm are more likely

[1]For example, the Excessive Pay Shareholder Approval Act (May 2009) would have required a 60% shareholder approval if an executive received more than 100 times the average salary within a firm.

[2]The report of the European Commission (2011) also specifically raises the question if the remuneration policy and report should be subject to a mandatory shareholders' vote, whether advisory or binding.

[3]For example, Belgium, the Czech Republic, Denmark, Finland, France, Hungary, Latvia, the Netherlands, Norway, Portugal, and Sweden have introduced laws on say-on-pay with partially binding elements.

to fear that shareholders "hold them up." Shareholders recognize that ultimately their own "piece of the pie" will be smaller when such specific investments are not made. In the present case, as we will make clear, the new regulation leads to situations in which shareholders vote on bonuses for management effort and performance in the elapsed year. More generally, contracting becomes more complicated and uncertain. If CEOs expect that they will not receive the full returns on their firm-specific investments, their ex-ante incentives to engage in such efforts are diminished.[4] This issue is not only of theoretical relevance, but also of a high significance to practitioners. In a recent interview, Dennis Nally, head of the global consulting firm PwC, warns that many firms in Switzerland are very concerned about the initiative, exactly because the previously described hold-up problem will make it hard to find skilled managers.[5] *Hypothesis 1*, therefore, states that the value impact of binding say-on-pay is more negative in firms where specific investments by CEOs are more difficult or more important to secure.

Second, say-on-pay may better align shareholder and manager interests and improve governance and performance. Allowing shareholders to have a say in executive pay may help to reduce the agency costs between executives, directors and shareholders, resulting in more efficient compensation contracts and thus add value to the firm. To avoid the embarrassment of a low approval vote on executive compensation, management may be more willing to start dialogues with shareholders and listen to their concerns. *Hypothesis 2*, therefore, states that the value impact of binding say-on-pay is more positive in firms where alignment is currently poor. This channel partially features in existing work on advisory say-on-pay; we extend the existing literature by conducting the first study of implications of *binding* say-on-pay and by

[4]This is true even if ex-post renegotiation is costless and efficient; see Grossman and Hart (1986) and Hart and Moore (1990) for seminal work on the hold-up problem. If renegotiation leads to disappointment and psychological costs ex post, this has additional distortionary implications (Hart and Moore, 2008).

[5]*NZZ am Sonntag*, November 11, 2012. This concern is shared by the trade association SwissHoldings, who warn in a recent newspaper article that the initiative's demands considerably harm the competitiveness of the Swiss economy as new talent will be hard to recruit (*Neue Zürcher Zeitung*, November 13, 2012).

considering a broad range of proxies for alignment benefits.

We study the announcement effects of the Anti-Rip-Off-Initiative.[6] Importantly, the announcement that enough public support in favor of the initiative was gathered to enforce a national vote came suddenly and was hardly predictable. This setting is exceptional, especially compared to the standard parliamentary vote situation where the date of the vote as well as the distribution of power in favor or against the issue is usually known in advance. Moreover, the Swiss stock market is highly liquid, allowing information to be reflected in market prices efficiently. Despite some caveats, which we discuss in the text, we believe that the announcement of the initiative overall provides a particularly clean experiment to identify the causal impact of binding say-on-pay and to test the two hypotheses.

Evaluating the stock market reactions to this event, we find that 70% of firms reacted abnormally negatively. In the three days around the event, the average cumulative abnormal return among the largest 100 firms is a highly statistically significantly negative 1.88%. For a sample of matched firms from the German market, which was unaffected by the initiative, we find an abnormal return of -0.10% (-0.03%) for a sample of CDAX firms matched according to stock return correlation (industry-size match). In both cases, the reaction of the German firms was statistically indistinguishable from zero.

While it is easy to think of factors that affect all firms and that therefore play a role for the general level of the reaction,[7] our primary interest is with the cross-sectional variation in reactions of Swiss corporations. As a novelty in the empirical literature on shareholder rights legislation, we consider various tests of the idea that enhancing shareholder power may

[6] The analysis of the value effects of regulatory changes confronts the researcher with difficult issues. It may take a long time for the costs and benefits of a regulatory change to materialize, and the actual changes in firm policies may be difficult to observe for the researcher. However, given rationality in the marketplace, the effect of an event will be immediately reflected in asset prices, an insight that has been used in many studies of the effects of regulation since Schwert (1981). With proxies capturing the variation in potential benefits and costs of the new regulation for companies, one can then also test hypotheses regarding differential effects of the regulation on firms even without observing actually changing firm policies.

[7] For example, shareholders may have a general aversion against changing rules of the game.

worsen hold-up problems and distort firm-specific investment incentives of CEOs (*Hypothesis 1*). These problems are likely to be particularly accentuated in firms where CEOs have opportunities and incentives to invest in general human capital and thus improve their outside options or where a high volatility in the line of business makes contracting difficult in general. While there is no obvious direct measure of the intensity of the hold-up problem, we propose three (largely uncorrelated) groups of proxies: *First*, shareholders of firms that use only cash bonuses – which would be subject to an ex-post shareholder vote[8], – may especially worry about a distortion of the ex ante incentives for executives. *Second*, shareholders of firms with younger CEOs and those with CEOs of a shorter tenure at the respective firm are likely to worry more that CEOs will have diminished incentives to make firm-specific investments; these CEOs would be more inclined to improve or exercise their outside options. *Third*, shareholders of firms with higher uncertainty concerning their annual sales or costs will find it more difficult to contract with management efficiently as more contingencies would have to be planned for, which is difficult under the new regime. Supporting the prediction of *Hypothesis 1*, we find that stock price declines were more pronounced in these three groups of firms.[9]

Hypothesis 2, regarding the alignment benefits of binding say-on-pay, also receives support. Firms which outperformed size- or risk-based benchmarks in the past experienced particularly substantial abnormal stock price drops, while poor performers reacted relatively more positively. Also, the stock prices of firms that paid their CEOs amounts close to the estimated normal salary tended to drop the most during the event, whereas firms where abnormal executive pay was either highly positive or negative only moved slightly.

Importantly, we also find that the portfolios which we compare (for example, firms with young CEOs vs. those with older CEOs, or those high performance vs. those with low

[8]For example, when the board wishes to reward extraordinary performance in the previous year.

[9]Other theoretical models predicting limits on optimal shareholder control include Allen, Carletti, and Marquez (2009) and Cohn and Rajan (2012). We discuss later the extent to which the evidence can be partially explained by these models.

performance) exhibited parallel trends in abnormal returns *before* the event, emphasizing its causal impact.

Moreover, while the initiative also contains some other provisions related to corporate governance, we document that none of them offers significant explanatory power for the cross-section of firm reactions. This is consistent with the fact that the public discussion and media coverage of the initiative almost exclusively concerned its say-on-pay content.

1.1. Contribution to the literature

This paper contributes to the literature on the empirical effects of shareholder power on shareholder value. In the context of say-on-pay, Cai and Walkling (2011) first recognized the potential of evaluating shareholder reactions to say-on-pay using an event study. They find neutral to slightly positive stock market effects of *advisory* say-on-pay, with positive outcomes in firms that paid their CEOs large excess compensation.[10] In the more general literature on shareholder power, Larcker, Ormazabal, and Taylor (2011) document negative market reactions to legal developments that suggest higher probabilities of governance and executive pay regulation. By contrast, Becker, Bergstresser, and Subramanian (2012) and Cohn, Gillan, and Hartzell (2011) find that developments suggesting a possible increased proxy access for shareholders in the future resulted in positive stock price reactions for firms where shareholders were more likely to take advantage of that access. Cuñat, Gine, and Guadalupe (2012) establish that when shareholders choose to adopt a provision that shifts power to them, this causes a positive shareholder value effect; this effect is stronger, for example, in firms with more antitakeover provisions. While these contributions are the most closely related to out work, we are well aware of the much broader focus of the literature on

[10] In a laboratory experiment, Göx, Imhof, and Kunz (2010) show that while advisory say-on-pay votes do not distort investment decisions, binding rules do so and may thus impair shareholder value.

shareholder power.[11]

Our analysis adds to this existing work by offering a combination of several features: *First*, we consider jointly a broad range of factors that explain stock price reactions. The main innovation is that we document that shareholders appear to consider a trade-off: They welcome binding say-on-pay because it helps them reign in agency costs, but they also anticipate hold-up problems when they have too much power. This confirms a prediction that so far has only been documented theoretically. *Second*, this paper focuses on the so far only qualitatively assessed *binding* say-on-pay, as a policy alternative to advisory say-on-pay. This offers us an opportunity to consider *Hypothesis 1*, and provides a sharper test of *Hypothesis 2* as alignment benefits are likely to appear more distinctly in this more stringent regime. *Third*, the analysis uses a particularly clean event. The announcement of the initiative's success is surprising, and the evidence on parallel trends in subsamples of interest before the event provides additional support for a causal interpretation of our findings.

2. Legislative setting and the binding say-on-pay initiative

To provide a better understanding of the setting in which the event study is conducted, we first describe the political environment that surrounds it. Second, we describe the major demands of the binding say-on-pay initiative.

[11]For work covering various aspects of say-on-pay see, e.g., Bainbridge (2008); Bebchuk and Fried (2004); Davis (2007); Deane (2007); Ertimur, Ferri, and Muslu (2011); Ertimur, Ferri, and Oesch (2012); Ertimur, Ferri, and Stubben (2010); Greenstone, Oyer, and Vissing-Jorgensen (2006); Grundfest (1993); Lo (2003); Thomas and Cotter (2007), and Thomas, Palmiter, and Cotter (2012). Conyon and Sadler (2009) and Ferri and Maber (2012) look at the impact of legislation on executive pay and shareholder activism outside the U.S. On shareholder activism more generally see Gillan and Starks (2000) and Gillan and Starks (2007).

2.1. The Swiss legislation process

The Swiss political system knows two common ways of enacting new laws (see Klöti, Knoepfel, Kriesi, Linder, Papadopoulos, and Sciarini (2007) for a more detailed summary of the Swiss system). One way is through a consensus decision between parliament and senate. The second way is through the public itself, by means of an initiative which can be started by every Swiss citizen. Switzerland has a lively tradition of direct democracy (see, for example, Frey (1994)). If an initiative receives the backing of at least 100'000 Swiss citizens (about 2% of the electorate of around 5'000'000) within 18 months, it must be put on the agenda for a national vote. In case the public vote supports the initiative, it will turn into an amendment to the Swiss constitution. The fraction of public initiatives that eventually pass the popular vote has been increasing in recent years.

2.2. Content and design features of the initiative

We consider the so-called "*Initiative gegen die Abzockerei*" ("Anti-Rip-Off-Initiative," "Fat-Cat-Initiative"). This initiative was launched by entrepreneur Mr. Thomas Minder. According to the initiative's text, it is proposed "to protect the economy, private property and the shareholders," making it a reasonably well suited event to study shareholder reactions to it. On February 26, 2008, the announcement was made that the threshold of 100'000 signatures in favor of the initiative had been collected. (We discuss the media coverage below.) Unlike many initiatives that are rather a general call for action to parliament and senate than original proposals to turn into law, the present initiative had a clear program that it aimed at turning into legislation. It offered a specific text to be adopted as law, discussed below. The fact that the initiative only represents a step toward a possible law implies that by studying stock market reactions to the initiative we likely underestimate the true economic impact it

would have upon enactment.[12]

The initiative affects all public Swiss limited liability companies. It requires a *binding* annual vote on total compensation (the sum of all pay components, such as fixed and variable pay) for each of three groups: the board of directors (BOD), the executive board (EB) as well as the advisory council. It is important to understand the particular design of the planned total compensation vote. The shareholders vote ex ante on the total amount of the different compensation packages of each body, as proposed by the firm's BOD. Furthermore, and critically, they have the right to vote ex post on all compensation that is supposed to be paid in excess of what has been approved at the previous general assembly. This latter point creates a link to the structure of compensation. For example, in a typical case shareholders may approve an equity plan as a part of base pay (where the amount approved is determined according to some valuation model) and a cash bonus pool for management for the coming year that covers the payout at target performance. For the equity plan, no extra vote will be necessary in the following year; the value of equity may simply go up or down. To the extent that the BOD wishes to hand out bonuses covered by this bonus pool, no additional vote is necessary ex post either. However, at the end of the year, if the BOD wishes to grant higher bonuses for the elapsed year – for example, because it perceives that senior management has made particularly successful efforts during the year – the difference needs to be approved by shareholders ex post. The initiative, therefore, is as much about pay levels as it is about the pay structure.[13]

Moreover, contracts with new management would be conditional on their pay packages being approved at the next general assembly, with obviously high uncertainty for management

[12]On February 26, 2008, the probability of the initiative passing into law quickly was seen as substantial and serious enough to catch the attention of the stock market participants. Subsequent political discussions have delayed a vote on the initiative (much like it took more than three years for the 2007 U.S. House Say-on-Pay Bill to find its way into law in the form of the Dodd-Frank Act in 2010). The vote on the initiative is scheduled to take place in March 2013.

[13]This design distinguishes the initiative from other proposals that call for a one-size fits all cap on compensation or a restriction on the ratio senior management compensation to salaries of average employees.

and the board. (One interpretation of the initiative is that if the incoming management's compensation package is similar to the leaving manager's package, the previously approved package may be used for the incoming management as well.)

In case shareholders do not accept any compensation proposal at the annual meeting, management has to schedule a new assembly to vote on a revised proposal; this is an arguably very expensive outcome that hurts the company's reputation. To avoid the latter, a firm's board has to ensure ex ante that its proposals will be supported by a majority of shareholders. This tight interaction with shareholders is a resource-consuming, ongoing process.

Moreover, the initiative closes all known loopholes to keep remuneration proposals from annual votes. For example, it prohibits companies to delegate a firm's management to a foreign company.

While the public discussion and media coverage of the initiative almost exclusively concerned its say-on-pay content, we note that the initiative also contains some other provisions. Our setting provides an opportunity to test whether the market reacted to these provisions. Specifically, performance-related pay to the BOD and the EB as well as other benefits (loans, pension benefits, etc.) need to be set in the firm's articles of association and can only be altered through a vote of the general assembly. The initiative also prohibits any kind of termination pay or advance payments to the BOD or EB. Other requirements pertain to the election modes of the BOD and the compensation committee. As we will see, the cross-sectional variation in market reactions is not explained by these elements.

The full text of the initiative can be found in Supplementary Appendix A.

3. Empirical strategy and data

3.1. Event study

We follow standard practices (Kolari and Pynnönen, 2010; Kothari and Warner, 2007; MacKinlay, 1997). Based on the event described in the Section 3.2, we define an event window that

spans ±1 day around the event-day. For the length of the estimation-window, we choose the well-established duration of 250 trading days ending two days before the event.

To calculate abnormal returns (AR), we apply the commonly used market model:[14]

$$R_{i,t} = \alpha_i + \beta_i R_{m,t} + \epsilon_{i,t}. \tag{1}$$

The difference between the effectively observed return ($R_{i,t}$) and the predicted normal return ($\widehat{R_{i,t}}$), estimated by using Equation (1) is the *abnormal return*, and *cumulative abnormal returns* (CARs) are the sum of the abnormal returns in the event window. In Equation (1), $R_{i,t}$ is the risk-free rate adjusted return of company i on day t ($r_{i,t} - r_{f,t}$), $\epsilon_{i,t}$ is a zero-mean disturbance term and α_i a stock specific constant. We also need to choose $R_{m,t}$, the daily risk-free adjusted return of the market at date t. For the main analysis, we follow the most widely used approach in event studies, using a national market index, the Swiss Performance Index (SPI). Thus, β_i is the sensitivity measure of stock i to movements of the SPI.[15] We alternatively take the view of a globally integrated market and conduct our analysis using the Dow Jones Global Total Stock Market Index as the market return. All our results hold (both qualitatively and quantitatively) when assessed with a global benchmark.

When comparing mean CARs of portfolios formed according to relevant characteristics of interest, for the main presentation, we use the resulting CAR-variance to draw interference.[16] We also employ an adjustment to the Boehmer, Musumeci, and Poulsen (1991) test statistic,

[14] In short-run event studies, the gains from employing multifactor models for event studies are limited. See, for example, the discussion in MacKinlay (1997), p. 18.

[15] The analysis for the sample of comparable German firms was conducted likewise. Normal returns were calculated based on the CDAX index.

[16] When testing the impact of legislative events on a cross-section of companies, event-time clustering (a common event window for companies) can potentially complicate inference because it implies a violation of the assumption of independence of abnormal returns in the cross-section of analyzed firms (Bernard, 1987). However, even for our basic testing procedure, this problem is typically much attenuated in studies like ours that use very short event windows in connection with daily return data (see, for example, Kothari and Warner (2007)).

proposed by Kolari and Pynnönen (2010).[17] By taking into account the average sample cross-correlation of abnormal returns in the test-specific variance, they show that their adjusted test statistic not only stays robust in case of an event-induced variance increase, but also to event-time clustering.[18] (For details, see Supplementary Appendix B.)

Finally, we further follow proposals by Campbell, Cowan, and Salotti (2010) and Kolari and Pynnönen (2010) and complement the parametric tests mentioned above with a non-parametric test, in our case the generalized sign test (Corrado and Zivney, 1992). The generalized version of the sign test was calibrated according to the binomial distribution of positive and negative abnormal returns, during the estimation window, of the portfolio of stocks under consideration. Campbell, Cowan, and Salotti (2010) show that this test generally performs better compared to parametric tests as it does not rely on assumptions regarding correlations (and is, as such, free from the clustering issue), yet has a drawback in case the event induced variance change is large. Since the variance increase in our sample is only 30% instead of the doubling assumed in their test environment, we believe that the generalized sign test is a reliable complement to the parametric tests.

3.2. The event and its coverage in the media

In every event study, it is important to carefully examine and define the date at which the event to be analyzed took place. We conducted a national keyword-search in the news-database of LexisNexis for the time period of July 2006 to March 2010, the timeline during

[17] Both test statistics account for event-time clustering by using scaled cumulative abnormal returns (SCARs), as suggested by Patell (1976). Scaled abnormal returns reduce noise by weighting abnormal returns by the inverse of their standard deviation and hence make it more likely to detect the true statistical significance of the data. The test proposed by Boehmer, Musumeci, and Poulsen (1991) not only takes into account event induced variance changes, but also has better properties vis-a-vis the standard test to deal with event time clustering.

[18] As with all test-statistics based on SCARs, the authors point out that it is important to only consider SCARs to detect statistical significance of abnormal returns, but to rely on standard CARs for the interpretation of economic effects. Hence, when comparing the difference in reaction between various portfolios, we rely on the measures of basic CARs.

which the initiative has been developing.

The main results of this search are collected in Table 1, and we discuss them briefly here. The initiative was initially mentioned in the first week of August 2006, officially verified in mid-October 2006, and the collection of signatures started on the last day of October 2006. As these first three steps all carried a lot of uncertainty about the outcome and implication of the initiative, it seems very unlikely that they had a significant impact on the stock market.

TABLE 1 ABOUT HERE

The event we focus on in this paper, on February 26, 2008, was the announcement that the threshold of 100'000 signatures in favor of the initiative had been collected. This event was hardly predictable for market participants since there was no publicly available signatures count.

The news were released shortly before mid-day and communicated widely through various channels, i.e., radio, television, news networks, etc. The coverage was further extended on the following day by the print media.

According to Swiss press sources, the announcement was chosen to be released right before the reporting season of the largest Swiss corporations started. By doing so, the promoter of the initiative, Mr. Minder, aimed at increasing the pressure on companies to voluntarily introduce advisory votes. This is another indication that the news release was new to the market, as this strategy could not have had the hoped-for impact otherwise.[19]

The news were to some extent also picked up internationally; for example, after having posted the announcement by the Swiss News Agency (SDA) in German in the early afternoon, Bloomberg further reported on the initiative's success in the late afternoon in English under the heading "*Swiss May Vote to Expand Shareholder Rights Over Executive Pay.*"

[19] In a newspaper article seven months prior to the submission of the signatures, Mr. Minder told the press that he had not anticipated that collecting 100'000 signatures would be so difficult. Moreover, he stated that the following months of the collection period will be very difficult (newspaper *Blick*, June 23, 2007).

In sum, these facts make the present event an attractive one from a methodological point of view. Nonetheless, we emphasize two factors that are important for the interpretation of the analysis. *First*, although Bloomberg's coverage suggests that international investors are likely to have been informed about the event, media coverage in Switzerland was much more pronounced than internationally. For example, we note that the Factiva database (which does not cover Bloomberg) does not show additional news coverage in U.S. media on the event date. We address this factor in Section 4.1 and argue that if anything this introduces a bias against finding a significant market reaction.

Second, while we are primarily interested in the cross-sectional variation in abnormal returns on the event day, it is still of interest to know whether other events may have influenced the general mood in the markets around the announcement. One related event concerns the bank UBS. On February 26, 2008 a Swiss government official told the press that he believes UBS would not need government support to overcome the crisis. A day later, UBS held a special shareholder meeting at which a raise in equity capital was agreed. Another possibly relevant event took place on February 24, 2008, when a corporate tax reform (the "*Unternehmenssteuerreform II*") was accepted in a referendum by the Swiss electorate. We address these two events in Section 5.4.

Particular events that potentially impact single firms specifically (e.g., earnings announcements), were controlled for separately.[20]

Overall, we expect that any statistically significant abnormal return in the event window can be attributed to the initiative.

[20]For the estimation-window, we also searched for news in connection to the initiative that may potentially lead to a biased event window return estimator. For our event, we could not identify significant news content that was directly connected to the legislation. We comment on one possible confounding event in the robustness section.

3.3. Data

Our initial sample covers all the companies listed in the Swiss Performance Index (SPI), the index of the overall Swiss market, during the event window. For the main analysis, we focus on the one hundred largest companies. Information is more quickly reflected in stock prices for large firms (Hong, Lim, and Stein, 2000; Hou and Moskowitz, 2005; Peng, 2005) and data more widely available. However, our results largely also hold in the full sample of 225 stocks. Some additional results we find in the expanded sample are reported in the robustness section.

To calculate firm-level stock returns, we use daily closing prices of the SPI constituent companies from the Thomson Reuters Datastream database.[21] We screen the data following the recommendations of Ince and Porter (2006).

The free-float adjusted market value (*Market Capitalization* in what follows), the total market value of the SPI companies,[22] other price data for the Swiss Performance Index (which we used to calculate the market return), trading volume, sales volume, cost of goods sold (COGS), the SPI size-segment indices (each SPI stock is assigned to either the small-size, medium-size, or large-size stock index), and the long-term Swiss government bond rate (a proxy for the risk-free interest rate) are also collected from Thomson Reuters Datastream. *Abnormal Trading Volume* is the difference between trading volume in the event window and the median trading volume of the respective firm in the previous year, taken as a percentage of the the median trading volume of the respective firm in the previous year. *Sales Volatility* (*COGS Volatility*) measures the standard deviation of a firm's sales (COGS) during the window of 2002 - 2007 and scales it by the average annual sales (for both variables) of the company during the same period. The scaling is necessary to account for the overall

[21]For the abnormal return calculation of the matched sample of German firms, we also use firm-level stock returns for the CDAX constituents as well as the CDAX index from the Thomson Reuters Datastream database. From the same source we obtain industry classification used for matching.

[22]In four cases where free-float adjusted market value was not available, we used total market value instead.

size of the firm. Return data for the SPI size-segment subindices are used to obtain each stock's size-index adjusted one-year performance (*Relative Performance*). Furthermore, we use weekly stock returns to calculate a risk adjusted performance measure, *CAPM Alpha*. CAPM Alpha is the residual from a one-year predicted return, based on a two year, quarterly rolling CAPM model return estimate, and the observed annual stock return.

Data on the firm's *Leverage*, measured as total debt to total assets, a CEO's *Tenure* at the current firm, and the *CEO Age* are obtained from Bloomberg.

Compensation data for 2007 is from PricewaterhouseCoopers (2008) for the largest 48 companies and expanded to the full sample by hand-collection.[23] Companies also document the *Cash Incentives*, which is the portion of variable compensation conveyed in cash (and not in equity). The data also cover *Other Payments*, the largest component of which are payments towards pension plans.

In the spirit of Bebchuk, Cremers, and Peyer (2011), we calculate abnormal compensation as difference between total compensation paid and remuneration granted by the average comparable firm (*Abnormal CEO Compensation* and *Abnormal Board Compensation*). The parameters for the prediction of normal compensation are estimated separately for CEOs and board members to account for their different status inside the firm with respect to remuneration. For CEOs, the prediction of the normal wage is based on the log of market capitalization, *ln(MCap)*, and on the one year, size-index adjusted firm performance, with a further control for executive turnover, *Months*, the number of months an executive worked in the firm during 2007, as well as *Dual*, a binary indicator stating whether the CEO holds

[23]Most companies provide company reports in the period January - March of the following year. As such, at the end of February 2008, strictly speaking, information on compensation in all companies in 2007 may not yet have been publicly available. Reliable compensation data for 2006 is not available for Switzerland, however. The Transparency Act requiring firms to disclose compensation data came into force only in 2007.

the position as chairman of the board at the same time:[24]

$$ln(\text{Comp})_i = \beta_0 + \beta_1 ln(\text{MCap})_i + \beta_2 \text{Relative Performance}_i + \beta_3 \text{Months}_i + \beta_4 \text{Dual}_i + \epsilon_i. \quad (2)$$

Based on the coefficient estimate from Equation (2), we predict total normal compensation for each executive. Abnormal compensation is then defined as the gap between predicted normal and effectively paid compensation. To construct the portfolios used in Table 4, individual abnormal compensation is aggregated by firm.

We also hand-collect, from firms' annual reports, the fraction of *Management Shareholdings* in the firm, whether a firm has a *Staggered Board*, which election procedure of board members a company employs (*Single Election* vs. in-corpore vote), whether a CEO has a severance agreement (*CEO Severance Agreement*), an agreement regarding termination benefits in case of a change of control (*CEO Change-of-Control Clause*) or a notice period that exceeds 12 months (*CEO Long Notice Period*). The variable *Largest Shareholder* captures the percentage of equity owned by the largest shareholder. The binary indicator variable *Company Event* is equal to one if a firm communicated its 2007 figures to the media within five days around the event window.

The summary statistics for all variables of interest are collected in Table 2. Due to the sometimes limited availability of certain data, the working sample is smaller for some parts of the analysis. Correlations for the most important variables are in Table 3. We note that the correlations of the variables of interest in the sample are overall very low.

TABLES 2 AND 3 ABOUT HERE

[24]The analysis was also conducted with further controls, such as industry fixed effects or leverage of the firm. Including these and other further variables did not improve the precision of the estimates which is why we include only the variables with the most explanatory power. For board members, we use the same approach but control for the number of members on the board, *Board Size*, instead of *Dual*.

4. Results

4.1. Reactions in Switzerland

An overview of the distribution of the individual three-day cumulative abnormal returns (CARs) for the full sample is provided in Figure 1. 70% of CARs were negative. As shown at the top of Table 4, the equal-weighted portfolio of all stocks in the Swiss Performance Index (SPI) showed an average abnormal return of -1.49%. The average CAR of the largest 100 stocks, on which our cross-sectional analysis focuses, was -1.88%. These average abnormal returns are highly statistically significant. The same pattern, both in terms of the cross-sectional variation and the overall average, also holds when using the Dow Jones Global Total Stock Market Index as the market portfolio. Here, the equal-weighted average CAR for the largest 100 stocks was -1.49%.[25] The effects are large, especially taking into account that the successful initiative alone does not guarantee that the proposal will ultimately pass the referendum and become law.[26]

FIGURE 1 ABOUT HERE

We noted earlier that the news on the initiative were predominantly spread in Switzerland. As previous literature has shown, the smaller a company, the more likely it is to have its investors in close proximity.[27] Thus, we expect investors of the relatively smaller firms to have been more exposed to the news regarding the initiative and hence to be the most responsive. If anything, therefore, including the largest companies in the sample is likely to bias the results against finding an effect of the initiative. The robustness analysis confirms that our

[25]It is a pure coincidence that this average is similar, up to two decimals, to the average abnormal return of the 225 SPI stocks stated above.

[26]The event-day abnormal return of -0.28% is of a similar size as the daily abnormal returns of -0.24% to 0.7% found in Larcker, Ormazabal, and Taylor (2011). The three-day CAR in our study is considerably larger than the three-day CAR of 0.25% found in Cai and Walkling (2011).

[27]No direct evidence of this hypothesis is available for Switzerland, but it holds powerfully in the U.S. (Coval and Moskowitz, 1999), and there is no reason to suspect this relationship to be different in Switzerland.

results hold strongly when excluding the largest 20 firms, for example, thus restricting the analysis to the sample of firms whose marginal investor is likely to be situated in Switzerland.

It is important to note that the reported reactions do *not* reflect the overall market reaction; they represent *equal*-weighted *abnormal* returns. (Naturally, and less interestingly, the *value*-weighted average is essentially zero.)

4.2. Reactions of a comparables German sample

Importantly, we compare the effect of the announcement on Swiss firms with the reaction observed in a sample of matched firms from Germany.[28] Overall, the average abnormal returns in Switzerland are strikingly different from those of the matched German sample. Specifically, we constructed two different portfolios of matched firms from the German CDAX index. The first portfolio matches the firms along the correlation of their daily stock returns during the estimation window. For each Swiss firm, we pick the German company with the highest correlated return. For the second portfolio, we match the firms along the criteria of industry (primary) and market value of equity (secondary). Figure 2, which plots the development of the cumulative abnormal return around the event date, displays the difference in reaction of the Swiss and the matched German portfolios. In Switzerland, on each of the three relevant days (the day before the event, the event day, and the day after the event), considerable negative abnormal returns were realized on average. In the days before and after the event window, cumulative abnormal returns remained fairly stable. In the matched samples of German firms, by contrast, the event had no statistically significant effect at all. The average three-day abnormal return in Germany is -0.10% for the correlation-matched portfolio and -0.03% for the industry-size-matched portfolio. In both cases, the event has no statistically significant effect. Moreover, while 70% of Swiss stocks show a negative abnormal return during the event, it is less than 50% for both matched samples of German firms.

[28] As the initiative covers all listed firms in Switzerland, the matched sample had to be constructed from foreign firms.

FIGURE 2 ABOUT HERE

4.3. Overview of the main analysis

We next turn to the main question of the paper: Do firm characteristics capturing variation in costs (due to potential distortions in firm-specific investments) and benefits (due to improved alignment) help explain the variation in CARs across Swiss firms? To answer this question, we use two approaches:

(1) We compare mean CARs across portfolios formed according to these firm characteristics. These results are presented in Table 4. This approach has the benefit that we can make use of the maximum number of observations for each variable.

(2) We regress CARs on the firm characteristics of interest, which allows us to hold certain important control variables constant. Baseline results for each variable of interest are in Table 5. Table 6 contains regressions that jointly consider Hypotheses 1 and 2 and include a larger set of control variables (which somewhat reduces the number of observations). Fortunately, our variables of interest are not highly correlated (cf. Table 3). As such, it is not surprising, but still reassuring, that the results we find in the portfolio analysis in Table 4 carry over to the regression results in Tables 5 and 6.

TABLES 4, 5 and 6 ABOUT HERE

Section 4.4 deals with Hypothesis 1, while Section 4.5 discusses Hypothesis 2. In Section 4.6 we document that the various portfolios we compare – for example, those with high and those with low expected alignment benefits – exhibit parallel trends in the time period *before* the event. Finally, Section 4.7 considers the findings when testing Hypothesis 1 and 2 jointly. (Section 5.3 analyzes the potential relevance of other features of the initiative that are unrelated to say-on-pay as well as the impact of shareholder structure.)

4.4. Hypothesis 1 – Distortion of extra-contractual investment incentives

Burkart, Gromb, and Panunzi (1997), Blair and Stout (1999), and Stout (2003), among others, develop the idea that shareholders may prefer not to be too powerful because with greater power comes a greater temptation to ex post expropriate those stakeholders that have made firm-specific investments. Burkart, Gromb, and Panunzi (1997) study optimal shareholder ownership dispersion; Blair and Stout (1999) and Stout (2003) deal with the relationship between the board and shareholders. Although their research does not explicitly cover the pay-setting process, their basic intuition extends to the present case.

We consider three arguments and corresponding proxies for why shareholders worry to a different extent about their CEOs' incentives to engage in firm-specific human capital investments. This approach circumvents the difficulty that actually observing CEOs' specific investments is rarely possible. Naturally, the informativeness of the hypothesis tests depends on the (untestable) strength of the link between the observable measures proposed and the true variable of interest, namely, extra-contractual investments that will change once the regulation is put in place. We aim to ameliorate this concern by studying three largely independent arguments.

First, consider the pay structure. As explained in Section 2.2, the time-line of how executive pay will be set according to the proposed law leads to potential distortions: Compensation packages (and, in particular, potential bonus pools) are agreed upon at the beginning of the year. If the board wishes to award extra bonuses after a year (which is especially the case if unanticipated effort and performance by management in the elapsed year were high), a new shareholder vote would have to be held at the next shareholder meeting. This is almost a prototypical case of the hold-up problem: Ex post, shareholders have little incentive to approve the awards.[29] The CEO, in turn, may anticipate this problem and, therefore, not

[29]In particular, the shareholders' incentives are considerably smaller than the board's: Boards of Swiss companies are explicitly charged to act for the benefit of the overall corporation. Also, their benefits from expropriating management are significantly lower than those of the shareholders'.

make the firm-specific investments that maximize firm and shareholder value. Importantly, we expect the resulting distortions to be greatest where executives are mostly compensated with cash bonuses. (According to the initiative, equity-plans need to be implemented in the articles of incorporation and from then on are simply executed.) Consistent with this prediction, Panel A.1 of Table 4 shows that the CARs were 2.5 percentage points more negative in firms that only use cash bonuses as variable compensation than in firms that use equity-based compensation.

Second, the time horizon of the manager plays a role. Younger CEOs have a relatively higher incentive, under binding say-on-pay rules, to invest in general rather than firm-specific skills than older CEOs because young CEOs wish to retain their option to secure a different position. Consistent with this argument, we find that firms with young CEOs reacted much more negatively to the say-on-pay initiative than those with older CEOs; see Panel A.2 of Table 4.

Relatedly, CEOs who have had a long tenure at the respective company are likely to already have acquired substantial firm-specific knowledge. By contrast, CEOs who have only relatively recently joined the company face the choice whether to engage in firm-specific or general human capital investments, i.e., whether to fully contribute to their current firm's fortunes or whether to at least partially work on their outside options. In Panel A.3 of Table 4 we find that shareholders of firms with CEOs in the shortest tenure quartile were more worried about the value consequences of binding say-on-pay: in this quartile CARs were about 1.75 percentage points lower in this quartile than in the other three quartiles, though the difference is statistically not highly significant.

Third, where uncertainty is high, it is more difficult to contract on all possible contingencies. Therefore, incompleteness of contracts becomes a major concern. The binding say-on-pay initiative may further exacerbate the ensuing hold-up problem. In line with this argument, Panels A.4 and A.5 of Table 4 show that stock prices of firms with higher-than-median demand or cost uncertainty exhibited 1.7-1.8 percentage points larger abnormal

declines than firms with lower-than-median uncertainty.

All these results are confirmed in the regression analysis, both when including the variables individually (Table 5) and when including them jointly together with other controls (Table 6). (We comment on the analysis including alignment benefits in more detail below.)

We note that some of the findings related to our proxies for the difficulty of sustaining firm-specific investment can also be explained by other theories. Specifically, in the model of Cohn and Rajan (2012) reputational concerns make managers reluctant to implement strategy changes. According to their hypothesis 1, board strength is optimally greater when the manager is young, but is invariant to age when reputational concerns do not matter anymore to the manager. This is consistent with the observation in Panel A.2 of Table 4. The Cohn and Rajan (2012) model can also be interpreted to rationalize the result regarding tenure in Panel A.3. Allen, Carletti, and Marquez (2009) provide a model in which overall firm value depends on the governance orientation of the firm (shareholder vs. stakeholder) and the main risk a company faces (demand vs. marginal cost uncertainty). Their central result is silent on the impact of changes in a firm's risk on the relative attractiveness of the two governance models. However, based on their predictions for shareholder vs. stakeholder firms, higher demand uncertainty and higher marginal cost uncertainty imply a smaller positive effect of a stronger shareholder value orientation in a certain parameter range. (These calculations are available on request.) Thus, in that range, their model is consistent with the findings in Panels A.4 and A.5 of Table 4.

Overall, it may well be that multiple forces are at work that drive the empirical facts we observe. Nonetheless, the extra-contractual investments framework is attractive because it provides a "brittle hypothesis:" It is a single model that makes several different predictions that could easily be wrong. Recall also from Table 3 that the various factors for which it correctly makes predictions are almost uncorrelated empirically (except, of course, age and tenure, and demand and cost uncertainty, respectively). None of the three independent predictions – regarding pay structure, time horizon of the manager, and uncertainty – is

rejected in the data. Moreover, neither of the alternative theories discussed in the previous paragraph predict the finding regarding the ratio of variable compensation paid in cash.

Summarizing, these considerations lead us to view the extra-contractual investments framework as particularly useful for adding to our understanding of shareholder reactions to enhanced shareholder power.

4.5. Hypothesis 2 – Alignment benefits

First, if management is not working in the interest of shareholders, firm-specific stock performance is likely to be poor. According to the hypothesis that binding say-on-pay helps improve alignment of managerial with shareholder interests, we should observe that firms with poor performance in the past benefit more from say-on-pay than those with the best performance.

In line with this prediction, the results in Panel B.1 of Table 4 display a negative relationship between the one year relative performance and the cumulative abnormal return.[30] Firms that had beaten the market on average over the past year generally dropped more than underperforming shares. As shown in Panel B.2 of Table 4, we find similar results for the risk-adjusted performance measure (CAPM alpha). In column (6) in Table 5 and in all regressions of Table 6, we find a strongly negative relation between past performance and the reaction to the binding say-on-pay initiative. (The results hold for both performance measures, but for expositional reasons are only shown for one.) These findings confirm that, indeed, binding say-on-pay is relatively more attractive for shareholders of firms that have

[30]While this resembles a classical mean-reversion phenomenon, we argue that this cannot explain the statistical significance of the cumulative abnormal return during the three day event window. First, as panel (c) in Figure 4(c) displays, the abnormal returns for firms in both past-performance quartiles, the lowest as well as the highest, exhibit a common trend prior to the event. Second, mean reversion is generally a gradual process. As such, it should be reflected by the sensitivity parameter of the market model and hence not lead to significant abnormal effects.

performed poorly than for those that have performed well.[31]

Second, a central point of interest is variation in share price reactions depending on the current pay level.[32] Due to a multitude of factors determining the absolute level of compensation, we focus on a standardized pay measure which is abnormal compensation. One interpretation of this measure is that, if a company overpays or underpays its management, it suggests poor governance.

We find that the middle 50% of firms in terms of abnormal CEO compensation on average lost in excess of a full percentage point more than the two corner quartiles, with the corner quartiles not showing a positive effect, see Panel B.3 in Table 4. This result, even though economically relevant, is not statistically significant on a regular level. However, when we control for the noise coming from firms that communicate their 2007 figures to the media around the event (c.f. Section 5.4), the difference is statistically significant (untabulated; the middle two quartiles drop 1.72% more than the corner quartiles, t-statistic of 1.81).

To capture the non-monotonic relationship in the regression framework, we control for abnormal compensation with a linear and a squared term. As Tables 5 and 6 show, the point estimates are of the expected sign, but not always significant. In untabulated regressions, we find very similar results for abnormal board compensation.

It is interesting to note some differences to the U.S. experience. When advisory say-on-pay became more likely to turn into law in the U.S., those firms with the highest abnormal pay benefited substantially, while the other companies reacted relatively neutrally (Cai and Walkling, 2011). The evidence from Switzerland instead tends to suggest that the market perceives firms currently operating with abnormal compensation close to 0 as being potentially forced to adjust to individually inefficient corporate policies under binding say-on-pay.

[31] This result is also consistent with an explanation based on extra-contractual investments. If performance was high in the past, this suggests that the firm had an able CEO who made substantial firm-specific investments. This CEO, or a successor, may be less likely to continue doing so under the new regime.

[32] Ertimur, Ferri and Muslu (2011) document that in the U.S. activists target firms with high CEO pay, but voting support is high and subsequent pay changes occur only at firms with excess CEO pay.

Third, a direct measure of alignment may also be found in the fraction of management shareholdings. The results in Panel B.4 of Table 4 suggest that firms with very little and very high managerial ownership fared relatively better than those with ownership that approximated the median. This (only borderline significant) result could reflect two effects: First, firms with very low ownership benefit from better alignment, which outweighs most of the interference costs of binding say-on-pay; second, firms with very high ownership do not benefit much, but also have very low compliance costs because managers and shareholders are often identical. In the regression setup, we also find some, albeit weak evidence of this u-shaped relationship between management shareholdings and CARs.

Fourth, consider leverage. On the one hand, shareholders may benefit from having more power in particular in firms with *lower* leverage because in these firms the agency costs of free cash flow are higher. On the other hand, shareholders of companies with *more* leverage may benefit more from say-on-pay because it is in these companies that shareholders have a particular interest in participating in structuring the compensation system.[33]

Panel B.5 in Table 4 and column (9) in Table 5 show that CARs are more negative for firms with low leverage. This finding suggests that the risk-taking alignment benefits are larger than the benefits of a reduction in the agency costs of free cash flow. This effect becomes insignificant, however, when controlling for proxies for the potential distortion of extra-contractual investment incentives.

Overall, we conclude that shareholders perceive some alignment benefits of binding say-on-pay.

[33]Specifically, in more highly levered firms, shareholders have a higher incentive to take asset risk, i.e., to engage in asset substitution (Jensen and Meckling, 1976). However, it is in such companies that CEOs may also be more reluctant to take risk because bankruptcy is very costly for a CEO in terms of reputation. Therefore, in highly levered firms, shareholders wish to grant higher incentives to take risk (Coles, Daniel, and Naveen, 2006). This is more easily done when shareholders have more power. From the shareholders' point of view, the board of directors may not sufficiently take the shareholders' preferences into account because the board, if it is acting according to the requirements of Swiss corporate law, is acting as a steward for the whole firm (i.e., including other stakeholders, in particular, bondholders).

4.6. Parallel trends of CARs before the event

By considering cross-sectional variation of abnormal returns during the event window, we have established that firms exhibited different reactions to the initiative. It is conceivable, however, that firms already exhibited different pre-event trends. This could lead to erroneous inferences regarding the causal effects of the event.

We examine this issue in Figure 3. We plot the daily level of cumulative abnormal returns during a window of 20 days (four trading weeks) before and 20 days after event. For presentational reasons, we choose two portfolio splits each for *Hypothesis 1* and for *Hypothesis 2*, but very similar results obtain also for the other sample splits.

FIGURE 3 ABOUT HERE

As can be seen, in all cases, cumulative abnormal returns of the two respective portfolios (for example, the portfolio with younger CEOs and the portfolio of firms with older CEOs) behaved very similarly *before* the event window. In fact, a t-test does not reject the hypothesis that the average trends of cumulative abnormal returns in the respective two portfolios before the event are equal.[34]

The similar *pre*-event trends are comforting and suggest that the sharp divergence of CARs *at* the event window was caused by the event.

4.7. Combining Hypotheses 1 and 2: Agency versus Hold-up

Table 6 considers all variables jointly. The proxies capturing the potential distortion of extra-contractual incentives are significant (almost) throughout. Of the variables capturing the alignment benefits of say-on-pay, past performance and abnormal CEO compensation are the

[34]Very similar observations hold when expanding the pre-event window to 30 days. An additional perspective is offered by testing, on each individual day, for the equality of the mean of abnormal returns in one portfolio (say, firms with younger CEOs) and the mean of abnormal returns in the other portfolio (say, firms with older CEOs). Out of 19 tested days, at most two days show significantly different abnormal returns for any of the considered variables. All these results are available on request.

most reliable predictors of shareholder reactions. Interestingly, in Table 6, the explanatory power of the regression increases substantially from left to right. In column (1), which includes only alignment benefits and some other firm-specific controls, the R^2 is 0.33; in column (5), which also includes measures of the importance of extra-contractual investment incentives, the R^2 is 0.43.

In sum, the central result revealed in this table is a so far empirically unexplored trade-off: The overall reaction of shareholders to enhanced power not only reflects the trade-off between alignment benefits and compliance costs, but also a trade-off between alignment benefits and a worsening of the hold-up problem.

5. Additional results and robustness

5.1. Firm size

The results in Tables 5 and 6 show that we generally obtain a strongly positive relationship between firm size and CARs throughout. There are several non-exclusive factors that explain this finding. Many of the very large Swiss firms had already introduced advisory say-on-pay in 2007, thus gaining experience with how to engage shareholders in this matter.[35] Furthermore, it seems reasonable to assume that fixed costs associated with binding say-on-pay will weigh less for the largest firms.[36] Moreover, because news coverage was more pronounced locally than globally, we do not expect large firms to react as strongly to begin with. Limiting the sample to only medium-sized firms, we confirm that our results continue to hold (not tabulated). Conversely, we also confirm that the results generally are very similar in the full

[35]Another indicator for this increased awareness of large firms is their significantly higher percentages of executive and board positions that have to be confirmed through individual elections.

[36]For example, large firms generally already have an established public relation department that is in constant contact with shareholders. The fixed costs may also be more subtle in the form of an increased effort by management to keep off large investors who aim at exchanging leading executive and board positions.

225 company sample, comprising the entire SPI.[37]

5.2. Shareholder structure

A measure of potential interest is shareholder structure. If there is only one shareholder with majority voting power, it is very unlikely that the new say-on-pay regulation will change anything in the corporate governance structure of this company. Absent this majority shareholder, uncertainty prevails due to lack of commitment ability of shareholders, leading to higher interference costs of say-on-pay.

However, the data provide no support of this idea (Panel C.8 of Table A-1 and column (8) in Table A-2).[38] While firms where a single shareholder owns a stake of 50% or more indeed tend to drop significantly less than firms with a more disperse shareholder base, which is consistent with the above argument, we find an unpredicted similar effect for the most dispersed firms.[39] Moreover, the difference between the firms with a dominant shareholder and more dispersed firms is not statistically significant, neither in the non-parametric tests nor in the regression analysis. This suggests that the shareholder structure of a company was perceived as not being related to the initiative's demands.

[37]The very smallest firms experienced less negative abnormal returns than the median-sized firms, in line with the idea that the very smallest companies are unlikely to be vulnerable to excessive shareholder-activism as the major shareholders are usually tightly involved in the firm's business.

[38]Only data of large shareholdings, above 5%, are comprehensively available in the year of the analysis. Since the activist shareholders that are known to wield significant power in Switzerland, for example, Ethos Fund, rarely hold more than 5% of a company, we cannot conduct tests, in the spirit of those of Cohn, Gillan, and Hartzell (2011), regarding whether reactions of firms with activist shareholders are more positive. In untabulated results, we find that firms' reactions did not vary significantly with the concentration of shareholdings among the group of large shareholders.

[39]This suggests that firms with a dispersed shareholder structure may benefit from the enhanced opportunities for shareholders to express their collective opinion on management pay.

5.3. Other elements of the initiative

The initiative contains a number of other provisions in addition to binding say-on-pay. While the public and policy discussion was almost exclusively about the say-on-pay component of the initiative, it is still possible that shareholders also reacted to some extent to these other proposals. To investigate this possibility, we compare market reactions in firms that currently use a provision that would be forbidden (or limited) under the initiative with the reactions in firms that do not use such a provision. The direction of any reaction would depend on the perception of shareholders regarding the value relevance of the provision. For example, if the market reaction in firms with a given provision is more positive than in other firms, this would be evidence that indeed shareholders perceived the issue as value-relevant and as connected with alignment benefits for them. If, by contrast, there is no significant effect, this can mean that–relative to the say-on-pay provisions–the market neglected the relevance of the initiative in this respect or that investors did not regard the issue as value-relevant.

We find that none of the non-say-on-pay provisions of the initiative is significantly associated with CARs. Specifically, proceeding in the order of the initiative's demands (see Supplementary Appendix A), we obtain the following results, which are summarized in Tables A-1 and A-2 of the Supplementary Appendix:[40]

(1) *Requirement of annual, individual elections of board members.* Reactions of shareholders were not different depending on whether a company currently already has individual elections in place and whether it has a staggered board (Panels C.1 and C.2 in Table A-1 and columns (1) and (2) in Table A-2). (2) *Possibility of electronic and distance voting as well as a requirement that pension funds vote in the interest of the insured.* It is difficult

[40]Some of the provisions will most likely not influence firms in measurable ways, and so we do not analyze them separately here; for example, very few board members have a consulting relationship with their own companies. Data for a few other items are not available. We do not have comprehensive data on other board seats of board members. Moreover, firms are not required to disclose (and do not in general disclose) whether they have in place a policy for sign-on payments for future executive hirings or for bonuses for management for certain transactions. Since market participants also do not observe such policies, their existence is very unlikely to explain the stock market reaction.

to find an appropriate proxy for this demand. A possibility is outlined in Section 5.2. If this provision was perceived as creating value for shareholders, we should see a relatively more positive reaction for smaller firms who should gain through an increased participation of their shareholders. On the other hand, firms with a majority shareholder are projected to show no reaction as any value creating provision would have already been implemented. The findings in Panel C.8 of Table A-1 and column (8) in Table A-2 clearly reject this hypothesis. (3) *A ban of severance agreements and change-of-control clauses.* This provision could be damaging for firms with young CEOs because these care most about their future career and for firms in an uncertain environment, so this provision of the initiative could partly explain some of the cross-sectional results we have documented. However, we see in Panels C.3 and C.4 of Table A-1 and columns (3) and (4) in Table A-2 that firms with severance agreements did not react differently than those without. A similar result applies for change-of-control agreements. Thus, the effect of CEO age on CARs we observed earlier is more likely to be driven by the hold-up channel of Hypothesis 1. (4) *Restrictions on credit, loans and retirement plans for management.* If investors worry about these restrictions, firms currently employing these instruments should react more negatively than others. We do not find any evidence in support of this idea (Panels C.6 and C.7 of Table A-1 and columns (5) and (6) in Table A-2). (5) *Restrictions on notice periods.* Firms where CEOs have employment contracts with notice periods of more than 12 months did not react more positively (which would support an alignment benefits argument) or more negatively (which would support an interference costs argument); instead, this feature of the initiative appears to have been irrelevant to shareholders (Panel C.5 of Table A-1 and column (7) in Table A-2).

(6) *Requirements regarding equity participation plans.* The initiative would require equity participation plans to be set in the articles of incorporation of the firm. If this provision had been behind the market reaction we would have observed more negative reactions in those firms that currently use such plans. By stark contrast, recall from the main analysis that we find that CARs were most negative in firms that did *not* use equity participation plans.

In sum, these results strongly suggest that the market reacted specifically to the proposed say-on-pay rules and did not interpret the initiative as a more generic push toward more regulation or other features that would affect corporate governance.

5.4. Robustness

First, we assessed the robustness of our results in the light of three events, two in the event window, and the third in the estimation window.

On February 27, a day after our event, UBS was able to raise USD 11bn of fresh capital in a special shareholder meeting. On the day before, then Swiss President Pascal Couchepin expressed his opinion that UBS would not need a government bailout. Even if this outcome of the shareholder meeting (which was hardly surprising as 599mn votes were in favor of and only 87mn votes against the capital injection) had moved the stock of UBS and perhaps the Swiss stock market as a whole, the cross-sectional results we obtain are unlikely to be explained by this event. Moreover, our analysis holds when restricting the event window to the day of the event itself. Furthermore, when we use the Dow Jones Global Total Stock Market Index (in which UBS has a small weight), we obtain the same results.

On Sunday February 24, 2008, the Swiss electorate accepted, in a referendum, a corporate tax reform (the "*Unternehmenssteuerreform II*"). The major points of the reform were aimed at supporting partnerships and small family businesses. A few elements were relevant for holding companies or owners with large stakes in individual firms, but have very limited impact on the regular firm listed on the SPI. (Financials did not react differently to the initiative than other firms.) Finally, the tax reform would allow companies to repay invested capital (including agios) tax-free, essentially allowing them to pay a special kind of dividend free of tax for the recipient. This rule change did not at all feature in the public discussion leading up to the vote, and few market participants seem to have understood the potential benefits of this new regulation. To the extent that the benefits were priced in, we would be

underestimating the negative overall effect of the say-on-pay initiative.

As for the possible confounding event in the estimation window, on February 10, 2008, a single newspaper released a short article claiming a successful end to the initiative's signatures collection. However, this claim was not officially confirmed, but rather discarded by an interview with the initiative's manager on the topic in the very same paper and day. Indeed, we found no abnormal reaction of the SPI stocks around this date. Shortening the estimation window so that it ends on February 7, 2008, also does not change the results.

Second, some firms announced their earnings around the event window, potentially affecting our results. The directional effect on the cumulative abnormal return is not clear, but test statistics including these firms are likely to be underestimated as announcements increase the sample's standard deviation. To investigate this effect, we defined a binary indicator variable showing whether a firm announced its 2007 results within five days of the event window. (Announcement effects usually fade quickly, making our choice of a five-day window a rather prudent one.) As seen in Tables 5 and 6, firms that announced their results in this window generally had more positive abnormal returns.[41]

Our regressions also show that CARs tended to be particularly negative where there was an abnormally large volume of trading, arguably driven by information processing by shareholders regarding the say-on-pay initiative's progress. We interpret this finding as reassuring evidence for the event's significance.

Third, we winsorized the event window CARs at the 5%-level to check for robustness against outliers. We find that our results stay unchanged.

[41] Omitting the firms with earnings announcements did not materially affect the results. Indeed, by excluding these firms, we reduce noise and hence improve the precision of our estimates. As mentioned above, for abnormal CEO compensation we now also find a statistically significant difference between the middle and the corner portfolios in the regressions equivalent to those in Table 5.

6. Conclusion

The present analysis uses an arguably clean event to investigate how shareholder power impacts shareholder value. Specifically, we first document that the unexpected announcement of a step toward a *binding* say-on-pay law in Switzerland led to significant stock price reactions. 70% of firms reacted abnormally negatively; the equal-weighted average abnormal return of Swiss firms was negative and statistically significant, while the average abnormal return of a matched sample of German firms was indistinguishable from zero. At first sight, the fact that shareholders reacted on average negatively to an enhancement of their power could be taken as evidence of Carl Fürstenberg's famous conjecture of "shareholder stupidity."[42] Careful analysis of the cross-sectional variation in reactions shows, by contrast, that the evidence is instead consistent with the view that shareholders rationally anticipate that say-on-pay has benefits and costs for them, and that they react most negatively where the costs are likely to outweigh the benefits.

These findings have important implications for the current policy discussion on how to design shareholder rights laws. Greater power provides shareholders with an enhanced ability to ensure alignment of managerial interests with shareholder value. However, in the natural experiment examined here, shareholders would obtain power in such a fashion (through the ability to vote on managerial compensation ex post) that this is likely to ex-ante distort extra-contractual managerial investments that are specific to the firm. By highlighting the resulting trade-off, we believe that this is one of the first papers to empirically support the argument, so far mostly presented in theoretical discussions, that it may be in the best interests of shareholders *not* to maximize their power. Policymakers should recognize that shareholders may do well to cede some control to directors (as they do under advisory say-on-pay, compared to binding say-on-pay).

[42]The German banker Carl Fürstenberg quipped: *"Shareholders are stupid and impertinent. Stupid because they give their money to somebody else without effective control over what that person is doing with it and impertinent because they ask for a dividend as a reward for their stupidity."*

Our study focuses on the impact of say-on-pay for shareholders. Some recent reforms in the executive compensation area also aim to benefit other stakeholders and society at large. The analysis here is silent on these broader implications, and future research is needed to address them.

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Figure 1. Individual cumulative abnormal returns around the event day

This graph shows the individual, non-winsorized cumulative abnormal returns (CAR) of the largest 100 firms in the Swiss Performance Index (SPI) in the event window. Abnormal returns are calculated with the market model and are sorted by size of the cumulative abnormal returns along the horizontal axis. The event-window covers the time span between a day prior and a day after February 26, 2008. On this day, it was publicly announced that the critical threshold of 100'000 signatures in favor of an initiative demanding binding say-on-pay in Switzerland had been collected. This requires the government to eventually hold a national ballot on whether the initiative should become constitutional law.

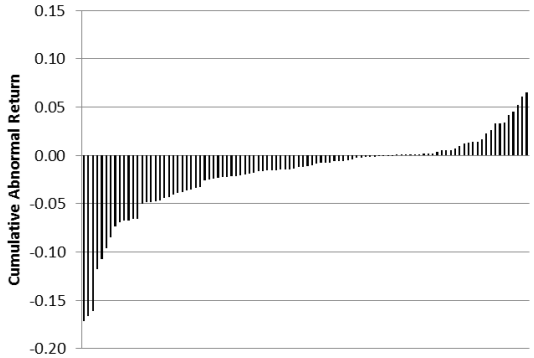


Figure 2. Average cumulative abnormal returns around the event day

This graph shows the equal-weighted average of cumulative abnormal returns for the largest 100 firms in the Swiss Performance Index (SPI, solid line) over time. The same analysis was done for a matched sample of German firms from the CDAX index. The portfolio of German firms was matched based on equity return correlation (dotted) or an industry-size match (dashed). Cumulation of the abnormal returns starts at t=-6. The vertical axis represents the average level of the cumulative abnormal return while the horizontal axis is measured in days relative to the event (t=0). The event window is marked by square brackets on the horizontal axis. Abnormal returns are calculated with the market model. The event window, [-1,+1], shows a cumulative abnormal return of -1.49% for the SPI firms and 0.42% for the CDAX firms. This cumulative abnormal return is the sum of the daily abnormal returns for SPI firms on day t=-1 (-0.61%), t=0 (-0.28%) and t=1 (-0.60%). For the equity correlation (industry-size) matched sample of CDAX firms, the abnormal returns are for -0.32% (-0.12%), 0.74% (0.51%) and -0.63% (-0.56%) for days t=-1, t=0 and t=1, respectively.

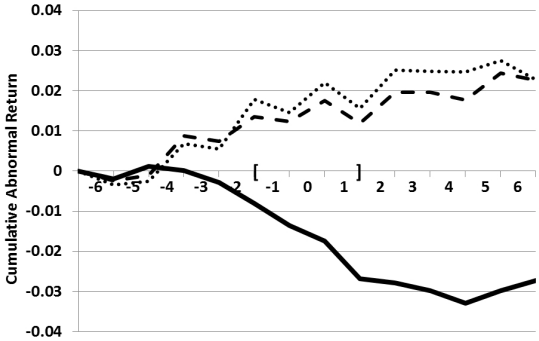
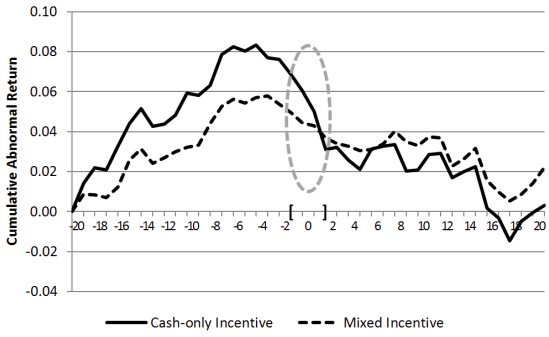


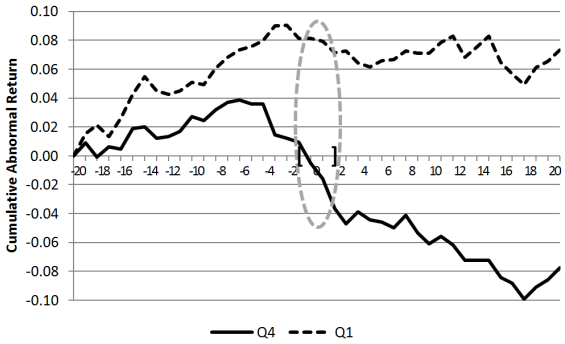
Figure 3. Trends of cumulative abnormal returns of subsamples around the event

Panels (a) to (d) show the daily level of cumulative abnormal returns for selected sample splits of the largest 100 stocks in the Swiss Performance Index during the 40 day window [-20,+20] around the event. Cumulation of the abnormal returns starts at t=-20. The vertical axis represents the daily level of the cumulative abnormal return while the horizontal axis is measured in days relative to the event (t=0). The event window is marked by square brackets on the horizontal axis. Abnormal returns are calculated with the market model. *Panel (a)* splits the sample according to the CEO's age in below median (solid) and above median (dotted) age. *Panel (b)* splits the sample according to the CEO's bonus structure into cash-only incentive (solid) and mixed incentive payments (dotted). *Panel (c)* shows the fourth (solid) and first (dotted) quartile of the sample in terms of the performance of a stock relative to the relevant size index. *Panel (d)* depicts the middle (solid) and corner (dotted) quartiles of the sample split according to abnormal CEO compensation.

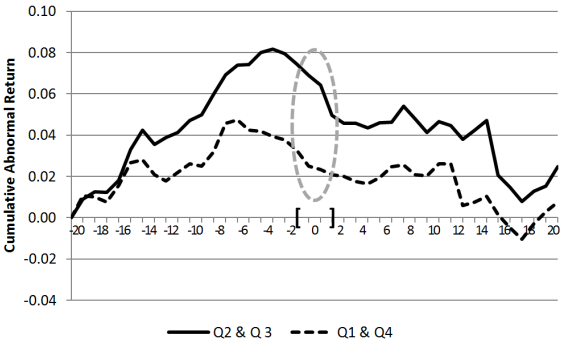
(a) CEO Age



(b) Cash Incentive Share



(c) Relative Performance



(d) Abnormal CEO Compensation

Table 1. Timeline of say-on-pay legislative efforts in Switzerland

Date	**Legislative events**	**Possible confounding events**
July 31 - August 6, 2006	A "Sonntags-Zeitung" article (08/06/2006) mentions that Trybol owner Thomas Minder has submitted the wording of the text of his "Fat-Cat-Initiative" that week.	a) On 08/03/2006 the European Central Bank (ECB) raised its interest rate by a quarter point to 3% as anticipated by analysts. Bank of England (BoE) surprisingly raising its interest rate by the same margin to 4.75%. b) The oil price was under turmoil that week because of war in Lebanon and uncertainty of the severeness of the Caribbean hurricane "Chris." c) Announcement of a below expectations net increase in employment in the US leading to believe that The Federal Reserve will not change interest rates after 17 increases in a row.
October 17, 2006	The Federal Chancellery verifies the initiative complies with legal requirements.	On 10/18/2006 the Federal Council of Switzerland had announced it entrusted five known experts the task to establish a federal audit supervisory authority.
October 31, 2006	Thomas Minder begins collecting signatures for a federal initiative.	Economic Committee of the National Assembly agrees to establish a Swiss Financial Market Supervisory Authority (FINMA) with 14 to 4 votes.
February 26, 2008 = Event	**Initiative committee submits the required 100'000 signatures.**	On 02/24/2008, a corporate tax reform lowering taxation of certain special types of dividend payments is accepted by the Swiss electorate.
April 2, 2008	The Federal Chancellery verifies the initiative as valid.	On 04/02/2008 the Swiss Market Index (SMI) gains 1.4% due to the extraordinary increases of the shares of the two major banks and in Tokyo the Nikkei reports a plus of 4.2%.
December 5, 2008	The Federal Council of Switzerland advises to reject the initiative and makes a so-called indirect counterproposal with an addition to the ongoing revision of the Swiss Code of Obligations.	On 12/05/2008 the Swiss Market Index (SMI) loses partially more than 3% and closes minus 2.09%. The German Stock Index (DAX) even loses 4%.
May 12, 2009	Judiciary committee of the Council of States tightens the indirect counterproposal and accommodates to the demands of the initiative committee.	No relevant confounding event found.
June 11, 2009	Council of States finishes debate over details of the counterproposal which is now less tight than the proposed form of the judiciary committee. The issue now returns to the national council.	The Associated Press reports that the US budget deficit has ascended to a new high in May and is expected to peak at the record high of 1.84 trillion dollar at the end of the fiscal year.

Table 2. Summary statistics for the main sample

This table displays summary statistics for the largest 100 firms in the Swiss Performance Index (SPI). Market Capitalization measures the market value of the free float on event day closing. Event Window Stock Return is the overall stock return during the three day event window. Relative Performance measures the gap between the observed stock return and the return of the size-appropriate index over a one year period prior to the event. CAPM Alpha measures the gap between the observed stock return and an estimated stock return based on CAPM for the year prior to the event. Sales Volatility is a firm's ratio of the standard deviation of sales to the average sales over the last five years. COGS Volatility is a firm's ratio of the standard deviation of cost of goods sold (COGS) to the average sales over the last five years. Leverage is measured as total debt to total capital. Company Event is a binary indicator equal to one if the firm communicated past year's accounting figures during a 10 day window around the event window. Abnormal Trading Volume is the difference between trading volume in the event window and the median trading volume of the respective firm in the previous year, taken as a percentage of the median trading volume of the respective firm in the previous year. Total CEO/Board Compensation is the sum of base and variable pay for the year 2007. CEO Cash Incentive Share is the share of a CEO's variable remuneration in 2007 that is paid in cash. Abnormal CEO/Board Compensation is measured as the difference between paid compensation and estimated normal compensation in terms of firm size and performance. All statistics for the board are reported including its Chairman. CEO Tenure is the number of years a CEO has been with the current company. CEO Severance Agreement is a dummy equal to one in case the CEO has a severance agreement. CEO Change-of-Control Clause is a dummy equal to one in case the firm grants the CEO severance payments specifically linked to a change of control in the firm. CEO Long Notice Period is a dummy equal to one in case the CEO's notice period is longer than 12 months. CEO Loans Outstanding is a dummy equal to one in case the firm is granting loans to the CEO. CEO Other Payments Share measures the ratio of salary payments other than fixed and variable relative to total compensation received by the CEO. The most prominent contribution to this share are payments towards pension plans. Largest Shareholder is the share the largest single shareholder holds in the firm. Management Shareholdings is the percentage of equity held by the management and board. Dual is a control for CEO-Chairs. Staggered Board is a binary indicator equal to one if the board is staggered. Single Election is a binary indicator equal to one if board members have to be elected one-by-one.

Variable	Mean	Std. Dev.	Min.	Max.	N
Firm Characteristics					
Market Capitalization (in Mio. CHF)	9'876.37	29'097.26	113.00	196'044.91	100
Event Window Stock Return (%)	1.71	4.33	-15.28	11.29	100
Relative Performance (in annual %)	11.32	69.48	-62.27	622.53	99
CAPM Alpha (in annual %)	-21.92	26.21	-72.03	99.55	91
Sales Volatility (%)	27.50	28.06	2.37	150.50	100
COGS Volatility (%)	23.19	53.03	0.66	388.41	76
Leverage (debt to total capital in %)	32.41	25.05	0	95.34	99
Company Event (binary indicator)	0.20	0.40	0	1	100
Abnormal Trading Volume (in %)	59.11	157.10	-65.39	967.40	100
Compensation					
CEO Total (in Mio. CHF)	4.25	4.49	0.48	22.28	91
CEO Variable (in Mio. CHF)	2.75	3.82	0	20.05	88
CEO Cash Incentive Share (in %)	57.47	34.50	0	100.00	97
CEO Abnormal (in Mio. CHF)	0.71	2.68	-2.67	11.61	85
Board Total (in Mio. CHF)	2.99	3.90	0.19	25.41	88
Board Abnormal (in Mio. CHF)	0.59	1.89	-1.10	11.29	88
CEO Attributes					
CEO Age (years)	53.51	7.69	37.00	82.00	97
CEO Tenure (years)	9.64	8.02	0.49	39.58	95
CEO Severance Agreement (binary indicator)	0.12	0.32	0	1	94
CEO Change-of-Control Clause (binary indicator)	0.23	0.43	0	1	94
CEO Loans Outstanding (binary indicator)	0.14	0.35	0	1	90
CEO Other Payments Share (in %)	0.09	0.09	0	0.47	90
CEO Long Notice Period (binary indicator)	0.06	0.24	0	1	100
Governance					
Largest Shareholder (in %)	27.40	23.14	0	99.40	100
Management Shareholdings (in %)	13.10	20.62	0	70.30	99
Dual (binary indicator)	0.15	0.36	0	1	88
Staggered Board (binary indicator)	0.59	0.50	0	1	92
Single Election (binary indicator)	0.56	0.50	0	1	91

Table 3. Correlations of explanatory variables

This table displays correlations of the explanatory variables for the largest 100 firms in the Swiss Performance Index (SPI). Variables are defined in Table 2.

Variables	1	2	3	4	5	6	7	8	9	10	11	12	13	14
CEO Cash Incentive Share	1.00													
CEO Age	-0.12	1.00												
CEO Tenure	0.00	0.32	1.00											
Sales Volatility	-0.09	-0.07	-0.04	1.00										
COGS Volatility	-0.08	-0.03	-0.04	0.77	1.00									
Relative Performance	0.01	-0.12	-0.07	0.07	0.01	1.00								
CAPM Alpha	0.27	-0.13	-0.02	-0.09	-0.03	0.95	1.00							
Abnormal CEO Comp.	-0.31	0.08	0.02	0.21	0.11	-0.05	-0.11	1.00						
Abnormal Board Comp.	0.04	-0.01	-0.07	-0.06	0.02	-0.06	-0.12	0.16	1.00					
Management Shareholdings	0.05	0.21	0.18	0.23	0.05	-0.05	-0.04	0.27	-0.01	1.00				
Leverage	-0.07	-0.07	0.01	-0.07	0.06	-0.20	-0.11	0.10	0.26	-0.24	1.00			
Largest Shareholder	0.12	0.04	0.16	0.04	0.05	-0.08	0.11	0.16	0.23	0.37	0.27	1.00		
ln(Market Capitalization)	-0.41	0.24	0.12	-0.25	-0.25	-0.02	-0.12	0.05	0.17	-0.11	0.03	-0.29	1.00	
Abnormal Trading Volume	-0.00	-0.04	-0.00	0.08	-0.01	0.08	0.34	0.07	-0.11	0.02	-0.02	-0.00	-0.08	1.00

Table 4. Market reaction to binding say-on-pay, analysis by portfolio-splits

This table displays cumulative abnormal returns (CAR) during the three day event window in quartile sorts for the variables of interest which we describe in Table 2. *All firms:* Average cumulative abnormal return of the Top 100 as well as all stocks in the Swiss Performance Index. *Panel A* concerns *Hypothesis 1*, regarding the distortion of extra-contractual investment incentives. *Panel B* concerns *Hypothesis 2*, regarding the alignment benefits. The last line of each panel tests for differences between portfolios of interest. For example, Panel A.2 tests for a difference between firms with CEOs of above median age and firms with CEOs below median age. Q1 is the bottom quartile, Q4 is the top quartile; these two quartiles are the corner quartiles. Q2 and Q3 are the middle quartiles. Stocks within quartiles are equal-weighted. Variable of interest (VOI) corresponds to the quartile average of the variable defined in the title of each panel. The t-statistic is calculated based on the variance of the unadjusted CARs as described in Supplementary Appendix B. KP is the test statistic obtained by conducting the adjusted Boehmer, Musumeci, and Poulsen (1991) test as proposed by Kolari and Pynnönen (2010). % neg is the share of negative CAR-stocks in the respective portfolio. The stars mark the level of significance based on the generalized sign test with levels: * 0.10, ** 0.05, *** 0.01.

All Firms					
	Obs.	CAR	t-value	KP	% neg
Top 100	100	-1.88%	4.53	2.13	70.00%***
SPI full	225	-1.49%	5.90	2.57	69.78%***

Panel A.1: CEO Cash Incentive Share (%)

Quartile	Obs.	VOI	CAR	t-value	KP	% neg
1 Lowest	25	12.7%	-1.20%	1.33	1.66	68.00%
2	24	43.1%	-1.60%	2.49	1.49	54.17%
3	23	74.8%	-0.98%	1.06	1.05	65.22%
4 Highest	25	100.0%	-3.74%	4.39	3.52	92.00%***
Q4 - Rest			-2.48%	2.54		

Panel A.2: CEO Age (years)

Quartile	Obs.	VOI	CAR	t-value	KP	% neg
1 Lowest	27	44.7	-2.73%	3.12	2.20	81.48%***
2	27	51.8	-2.89%	2.88	2.08	70.37%**
3	20	56.0	-0.91%	1.35	1.60	65.00%
4 Highest	23	63.7	-0.64%	1.05	1.26	60.87%
Median Split			2.05%	2.57		

Panel A.3: CEO Tenure (years)

Quartile	Obs.	VOI	CAR	t-value	KP	% neg
1 Lowest	24	2.4	-3.16%	3.51	2.96	83.33%***
2	25	6.0	-1.63%	1.84	0.72	60.00%
3	23	9.5	-1.56%	1.41	1.76	60.87%
4 Highest	23	21.3	-1.02%	2.36	1.55	73.91%*
Q1 - Rest			-1.75%	1.71		

Panel A.4: Sales Volatility (%)

Quartile	Obs.	VOI	CAR	t-value	KP	% neg
1 Lowest	25	6.8%	-0.23%	0.73	0.64	60.00%
2	25	14.7%	-1.48%	2.30	2.20	76.00%**
3	25	24.8%	-1.95%	2.85	1.86	68.00%
4 Highest	25	63.7%	-3.88%	3.08	2.36	76.00%**
Median Split			-1.74%	2.24		

Panel A.5: COGS Volatility (%)

Quartile	Obs.	VOI	CAR	t-value	KP	% neg
1 Lowest	19	4.0%	0.37%	0.62	0.13	42.11%
2	19	8.1%	-2.20%	3.40	2.54	78.95%***
3	19	14.2%	-1.26%	1.51	1.31	73.68%*
4 Highest	19	66.4%	-4.15%	3.35	2.63	84.21%***
Median Split			-1.79%	1.96		

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Panel B.1: Relative Performance (%)

Quartile	Obs.	VOI	CAR (%)	t-value	KP	% neg
1 Lowest	25	-26.6%	-0.97%	1.08	0.57	60.00%
2	25	-6.0%	-0.60%	1.03	1.15	52.00%
3	25	8.7%	-1.68%	3.71	2.62	80.00%***
4 Highest	24	71.6%	-4.61%	4.31	4.24	91.67%***
Q4 -Q1			-3.64%	2.61		

Panel B.2: CAPM Alpha (%)

Quartile	Obs.	VOI	CAR (%)	t-value	KP	% neg
1 Lowest	23	-48.3%	-0.26%	0.40	0.32	47.83%
2	23	-31.4%	-0.90%	1.51	1.13	60.87%
3	23	-17.7%	-1.14%	2.38	2.13	73.91%**
4 Highest	22	11.1%	-4.06%	4.30	4.37	95.45%***
Q4 - Q1			-3.80%	3.35		

Panel B.3: Abnormal CEO Compensation (Mio. CHF)

Quartile	Obs.	VOI	CAR	t-value	KP	% neg
1 Lowest	22	-1.16	-0.88%	1.81	1.70	68.18%
2	21	-0.67	-2.57%	2.51	1.90	76.19%**
3	21	0.21	-2.38%	2.11	1.66	66.67%
4 Highest	21	3.97	-1.51%	1.65	1.64	61.90%
Corner - Middle Quartiles			1.29%	1.43		

Panel B.4: Management Shareholdings (%)

Quantile	N	VOI	CAR	t-stat	KP	% neg
1 Lowest	28	0.2%	-1.23%	2.59	2.01	64.29%
2	22	0.6%	-1.30%	1.97	1.45	72.73%*
3	25	7.2%	-3.89%	3.47	2.66	80.00%**
4 Highest	24	45.7%	-1.20%	1.32	1.24	66.67%
Median Split			1.31%	1.56		

Panel B.5: Leverage (%)

Quartile	Obs.	VOI	CAR	t-value	KP	% neg
1 Lowest	25	4.0%	-3.70%	3.94	3.26	92.00%***
2	25	20.4%	-1.56%	1.80	1.26	64.00%
3	25	39.3%	-1.10%	1.25	1.13	48.00%
4 Highest	24	67.3%	-1.36%	2.77	2.05	79.17%**
Q4 - Q1			2.34%	2.20		

Table 5. Market reaction to binding say-on-pay, regression analysis I

Regressions in this table are based on the largest 100 firms in the Swiss Performance Index (SPI). The dependent variable is the cumulative abnormal return during the three day event window. The explanatory variables are defined in Table 2. Variables appended by quartile specifications are indicator variables with the indicator equal to one for the quartile stated. For example, Young CEO (Q1&Q2) is a binary indicator equal to one if the CEO is of below median age. The other variables are measured in levels. t-values are calculated based on robust standard errors and reported in brackets, with significance levels: * 0.10, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cash-only Incentive (Q4)	-0.019**								
	(-2.12)								
Young CEO (Q1&Q2)		-0.016**							
		(-2.08)							
Short Tenure CEO (Q1)			-0.013						
			(-1.58)						
Sales Volatility (Q2&Q3)				-0.017**					
				(-2.38)					
COGS Volatility (Q2&Q3)					-0.013				
					(-1.62)				
Relative Performance						-0.025***			
						(-7.22)			
Abnormal CEO Compensation							-0.003		
							(-1.17)		
(Abnormal CEO Compensation)^2							0.001*		
							(1.67)		
Management Shareholdings								-0.054	
								(-0.95)	
(Management Shareholdings)^2								0.170*	
								(1.82)	
Low Leverage (Q1)									-0.020**
									(-2.21)
ln(Market Capitalization)	0.004*	0.004**	0.005**	0.004**	0.002	0.006***	0.004**	0.006***	0.005***
	(1.90)	(2.02)	(2.44)	(2.13)	(1.08)	(3.26)	(2.00)	(3.08)	(2.88)
Company Event	0.024**	0.025**	0.025**	0.027**	0.035***	0.021**	0.021*	0.026**	0.022**
	(2.20)	(2.33)	(2.36)	(2.49)	(3.16)	(2.10)	(1.96)	(2.55)	(2.13)
Abnormal Trading Volume	-0.007*	-0.007*	-0.007*	-0.006	-0.009***	-0.006	-0.006	-0.007*	-0.007*
	(-1.75)	(-1.77)	(-1.73)	(-1.63)	(-2.71)	(-1.44)	(-1.13)	(-1.74)	(-1.75)
Constant	-0.042**	-0.041**	-0.053***	-0.042***	-0.030	-0.060***	-0.054***	-0.066***	-0.054***
	(-2.47)	(-2.40)	(-2.96)	(-2.67)	(-1.55)	(-4.06)	(-2.88)	(-3.90)	(-3.69)
Observations	97	97	95	100	76	99	85	99	99
Adjusted R-squared	0.180	0.176	0.162	0.185	0.267	0.340	0.107	0.208	0.201

Table 6. Market reaction to binding say-on-pay, regression analysis II

Regressions in this table are based on the largest 100 firms in the Swiss Performance Index (SPI). The dependent variable is the cumulative abnormal return during the three day event window. The explanatory variables are defined in Table 2. Variables appended by quartile specifications are indicator variables with the indicator equal to one for the quartile stated. For example, Young CEO (Q1&Q2) is a binary indicator equal to one if the CEO is of below median age. t-values are calculated based on robust standard errors and reported in brackets, with significance levels: * 0.10, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)
Cash-only Incentive (Q4)		-0.017*			-0.019**
		(-1.94)			(-2.21)
Young CEO (Q1&Q2)			-0.013*		-0.012
			(-1.68)		(-1.64)
Sales Volatility (Q2&Q3)				-0.023***	-0.024***
				(-2.76)	(-2.99)
Relative Performance	-0.022***	-0.022***	-0.022***	-0.021***	-0.019***
	(-6.13)	(-6.63)	(-5.76)	(-5.75)	(-5.74)
Abnormal CEO Compensation	-0.004	-0.005*	-0.005	-0.004	-0.005*
	(-1.46)	(-1.68)	(-1.65)	(-1.39)	(-1.81)
(Abnormal CEO Compensation)^2	0.000	0.001	0.001*	0.000	0.001**
	(1.42)	(1.59)	(1.82)	(1.48)	(2.01)
Management Shareholdings	-0.032	-0.030	-0.020	-0.004	0.009
	(-0.48)	(-0.47)	(-0.30)	(-0.07)	(0.16)
(Management Shareholdings)^2	0.130	0.135	0.101	0.108	0.087
	(1.27)	(1.32)	(1.00)	(1.17)	(1.01)
Leverage	0.013	0.015	0.013	0.010	0.012
	(0.63)	(0.75)	(0.64)	(0.52)	(0.68)
ln(Market Capitalization)	0.005**	0.004*	0.004*	0.004*	0.001
	(2.35)	(1.94)	(1.81)	(1.97)	(0.60)
Company Event	0.017	0.015	0.016	0.020*	0.018*
	(1.62)	(1.50)	(1.60)	(1.95)	(1.88)
Abnormal Trading Volume	-0.005	-0.006	-0.005	-0.005	-0.006
	(-1.02)	(-1.19)	(-1.13)	(-1.06)	(-1.36)
Constant	-0.064**	-0.050**	-0.047*	-0.045**	-0.014
	(-2.52)	(-2.31)	(-1.95)	(-2.04)	(-0.65)
Observations	84	84	84	84	84
Adjusted R-squared	0.327	0.349	0.340	0.386	0.429

Evidence of Excess Comovement in US Mergers*

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Abstract

This paper considers changes in market comovement of merging US firms. Comparing the expected to the actual post merger comovement, we find that the post merger beta exhibits excess comovement with the acquiring firm. This suggests that the firm's comovement is at least partly determined by its investors. We find that excess comovement is significantly greater in cash transactions, when target shareholders tender their entire stake, than in pure stock transactions. Additionally, we document that the excess comovement is greater when the target is included in the S&P 500 as a result of the merger.

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1. Introduction

Classical asset pricing theory predicts that in a frictionless market the return required by investors depends on the comovement of the firm's assets with the market. In an international context, there is evidence that the comovement changes significantly when the location of listing changes (Froot and Dabora, 1999, and Chan, Hameed and Lau, 2003) and when a company is acquired by a foreign firm (Brealey, Cooper and Kaplanis, 2010). These results suggest that stock comovement with the market is at least partly determined by the firm's investors and that international markets are segmented.

In the average merger, the majority of the target shareholders' stake is acquired and therefore the post merger shareholder base is predominantly comprised of the acquiring firm's shareholders. Given this, and if the market comovement is affected by the firm's investors, we expect the post merger market comovement to be shifted towards the acquiring firm. This paper examines US mergers and provides evidence that investors partially determine stock comovement by showing a significant shift in market comovement towards the acquiring firm.

We estimate the pre merger comovement of the target and the acquirer and use these estimates to calculate an expected post merger comovement. The expected post merger comovement is then compared to the actual post merger comovement. When the acquirer exhibits larger comovement with the market than the target (the prediction is asymmetric depending on the relative riskiness of the target and the acquirer), we find that the expected post merger comovement is 1.09 while the actual post merger comovement with the market is 1.18. This represents an excess comovement with the acquiring firm of 8.26 percent. Additionally, the implied effect on the target's market comovement is an increase in beta of 0.27 or 34 percent relative to the pre merger beta.

Given that investors affect market comovement, the degree of excess comovement should be increasing in the fraction of equity tendered by target shareholders. Therefore, cash

mergers (which imply that target shareholders do not retain any stake in the merged firm) should be associated with significantly greater excess comovement. For cash mergers, the difference between the actual and expected post merger beta is 0.20 (compared to 0.09 for the overall sample). In cash transactions, target comovement increases by 0.32 or 46.1 percent relative to the pre merger beta. In contrast, for 100 percent stock deals (when there is less exit), the excess comovement is statistically and economically insignificant.

Building on work by Vijh (1994), Barberis, Shleifer and Wurgler (2005) argue that there is a "habitat" of investors that invest in S&P 500 stocks. This implies that the firm's shareholders change as a result of inclusion into the S&P 500 and therefore the comovement with the S&P 500 increases. Given that there is a S&P 500 habitat, we expect the excess comovement towards a S&P 500 acquirer to be larger when a target firm is included into the index as a result of the merger. Our results support this conjecture and additionally we verify that our results are not driven by an index inclusion effect.

It is well documented that investors have a preference for particular firm characteristics like industry and geographic location.[1] Therefore, target shareholders that have a preference for a particular industry are more likely to sell their shares as a result of a cross industry merger than an intra industry merger. We find some support for this, in inter industry mergers (when the acquirer has a larger beta than the target) the excess comovement towards the acquirer's comovement is 11.71 percent while for intra industry mergers it is 3.77 percent and statistically insignificant. Similarly, there is no excess comovement in within state mergers while in intra state mergers the excess comovement is 9.03 percent.

The findings of this paper suggest that there is not only cross-border segmentation, but also segmentation along other dimensions such as index membership and geography. However, there are a number of possible alternative explanations for our results that we

[1]Empirically it has been documented that investors prefer stocks in their geographic vicinity (Coval and Moskowitz, 1999 and Huberman, 2001). Additionally, it has been shown that shareholders exhibit a preference for stocks from industries that they have experience from (Döskeland and Hvide, 2010).

have to consider. First, on average mergers are associated with increases in leverage and this could potentially explain the excess comovement.[2] However, for leverage changes to explain our results, it must be the case that leverage increases when the beta of acquirer is greater than the beta of the target and that leverage decreases when the beta of the acquirer is smaller than the target's beta, since in the first case we have a higher than expected post merger beta and in the latter a lower than expected post merger beta. In fact, for transactions in which the beta of the acquirer is lower than the target's, we find that leverage increases modestly. Additionally, we conduct multivariate sorts that illustrate that excess comovement is independent of the change in leverage. Finally, in our regression analysis, we find that the change in leverage is insignificantly related to the excess comovement and does not affect our results qualitatively.

Second, some mergers result in synergies which might transform the assets and therefore also the comovement of these assets with the market. However, for synergies to explain our results it must be the case that the synergy asset has a riskiness that is above that of the expected post merger beta when the acquirer has a higher beta than the target and vice versa when the beta of the target is greater than that of the acquirer. Further, it must be that the transformation of these is rather rapid since we measure the post merger beta over 100 weeks after completion. Finally, in regression analysis we verify that synergies are not driving our results.

Third, following completion it is possible that the riskiness of the assets of the target is transformed to become similar to the riskiness of the assets of the acquirer. However, this risk transformation needs to be rapid (see above). Additionally, it has to be greater in transactions that are associated with greater shareholder exit (e.g., cash deals). Furthermore, we consider the progression of the firm's post merger comovement and do not find evidence of a gradual transformation of the riskiness of the firm's assets.

[2]Ghosh and Jain (2000) study leverage increases in mergers. They find that leverage increases by a modest 6.3% on average. We find similar leverage increases in our sample (see Figure 2).

Prior work has provided evidence of segmentation by examining both returns and market comovements.[3] Concerning comovement, Chan, Hameed and Lau (2003) document a decrease in comovement with the Hong Kong and an increase in the comovement with the Singapore Stock Exchange following a change in listing from Hong Kong to Singapore. Most closely related to us, Brealey, Cooper and Kaplanis (2010) document cross-border segmentation by providing evidence of excess comovement in cross-border mergers. They find that following a merger, the comovement with the exchange where the acquiring firm is traded increases while the comovement with the exchange of the target company decreases. We build on their results by providing evidence of segmentation by considering mergers of US firms. Another paper that illustrates within-border excess comovement is Pirinsky and Wang (2006). They show that when firms change headquarters location they start comoving more with an index of firms in the geographic vicinity of their new headquarters.

The remainder of the paper is organized as follows. In Section 2, we describe our data sources, sample selection criteria and methodology. We start Section 3 by considering sorts illustrating the relation between the pre merger and post merger betas. We then verify these results using regression analysis. In Section 4 we show that our results are not driven by asset transformation and Section 5 concludes.

2. Data and Methodology

Our sample of mergers and acquisitions comes from the Securities Data Corporation (SDC). We only include transactions between firms listed on the NYSE, AMEX and Nasdaq. More-

[3]In terms of return segmentation, early work showed how investment barriers imply return premiums. The barrier to investment can be investment restrictions (Black (1974) and Stulz (1981)) or lack of information (Merton (1987)). In terms of empirical evidence, Hong and Kacperzyk (2009) show that "sin" stocks exhibit abnormal performance that cannot be attributed to traditional factors. Additionally, Sloan and Lehavy (2008) and Bodnaruk and Östberg (2009) show that firms with less recogntion (segmented firms in terms of investor awareness) have higher returns.

over, our sample covers the period from 1980 to 2008.[4] We only consider completed transactions where the target and acquiring company are publicly traded. Additionally, we require the target and acquirer to be different firms (i.e., we exclude all repurchases). This gives us a total of $8,411$ mergers. We obtain stock return data from the CRSP daily files (this reduces our sample to $6,160$).

In estimating comovement (see next section), we follow Brealey et al. (2010) and require 100 weeks of return data for the target and the acquirer prior to the run-up period and for the merged company after completion. This leaves us with $3,510$ deals.

Further, we only consider deals where 100 percent of the target company is owned by the acquirer after the merger. We only include targets which have a market capitalization above 50 million (Hackbarth and Morellec, 2008). In order to evaluate if the post merger comovement is biased towards the acquirer, we require that the targets assets to represent a non-insignificant proportion of total assets of the merged company.[5] Therefore, we only consider mergers in which the target company has a market capitalization that is at least 25 percent of the acquirer. Finally, we exclude deals which involve at least one financial firm (SIC code 6000 to 6999). This leaves us with a total of 712 deals.

To control for the change in leverage due to the merger we calculate the leverage change as defined by Ghosh and Jain (2000). Leverage is the fiscal year-end ratio of debt to total firm value. We measure debt as the book value of long-term debt (Compustat Item dltt) added to the debt in current liabilities (Compustat item dlc). Total firm value is the book value of debt added to the market value of equity. The change in leverage is defined as the difference in leverage between the fiscal year end before the announcement of the merger and the fiscal year end after the completion of the merger.

[4]SDC includes transactions from before 1980 and after 2008, but these transactions are excluded due to other restrictions.

[5]Brealey et al. (2010) do not have to implement such a restriction since they examine comovement with respect to different markets whereas we consider one market, but examine whether the acquirer determines a disproportionate share of the comovement.

We draw on Brealey et al. (2010) in calculating the synergies of the merger. Synergies are the market adjusted increase in market capitalization of the acquirer and target in the six weeks surrounding the announcement (three before and three after) as a percentage of the pooled firm.

Figure 1 describes the time line of our research design.

Insert Figure 1 here

We estimate the individual comovement of the acquirer, target and merged firm with the market (the value-weighted CRSP index) over the 100 week pre run-up period (acquirer and target) and over 100 weeks post completion (merged firm). To avoid confounding effects of news announcements and rumors, we exclude eight weeks prior (run-up) to the merger announcement (Schwert, 1996). This involves running the following weekly regression for the acquirer, target and merged company:

$$R_{j,t} = \alpha_j + \beta_j R_{m,t} + \varepsilon_{j,t}$$

where j is a firm index, $R_{j.t}$ is the return on the firm and $R_{m,t}$ is the return on the CRSP value weighted index. To reduce the effect of outliers, we winsorize our betas at the one and 99 percent level.

We calculate the expected merged beta as:

$$E(\beta) = \frac{MV_A}{MV_A + (1-\lambda)MV_T}\beta_A + \frac{(1-\lambda)MV_T}{MV_A + (1-\lambda)MV_T}\beta_T \tag{1}$$

where β^A and β^T are the pre merger comovements of the acquirer and target, respectively and MV refers to the market value of equity. If the acquiring firm has a significant toehold, the comovement of the target is already partly reflected in the comovement of the acquirer (Brealey et al., 2010). Put differently, if only a small stake is acquired in the target due to

the toehold then the comovement of the acquirer is not expected to change significantly. To control for this, equation (1) adjusts for the fraction of the target held by the acquirer at announcement (λ).

Table 1 presents descriptive statistics of our key variables.

Insert Table 1 here

On average, target companies are roughly half the size of acquiring companies. Target companies represent roughly 35 percent of total pre merger market capitalization. We can see that on average leverage increases from 23.91 percent pre merger to 31.74 percent post merger. Our descriptive statistics indicate that total synergies only represent a small fraction of the pre merger firm. Additionally, in most deals the acquirer does not have a toehold. The pre merger betas of the target and acquirer are similar and close to one. Turning to the expected beta ($E(\beta)$), as predicted, it is between the target and acquirer beta. Finally, the post merger beta (β_M) is greater than the expected beta which is consistent with a leverage increase.

We use SDC to classify the following methods of payment: cash, stock, mixed and other. Dummy variables *Cash*, *Stock*, *Mixed* and *Other* take the value 1 if the deal is only financed with cash, only with stock, a mix of both and if other methods of payment are used.

3. Empirical Findings

3.1. Univariate Analysis

This paper tests whether the investors contribute to the comovement of the firm with the market. To do so we examine mergers and acquisitions. Given that target investors exit

following the merger, the post merger comovement of the firm should be closer to the co-movement of the acquirer than expected. Additionally, the greater the fraction of target shareholders that leave as a result of the merger, the closer the post merger comovement should be to the comovement of the acquirer.

In this section we provide univariate analysis of the relation between the expected and the actual merged beta. Our central hypothesis is that the comovement of the merged firm is closer to the comovement of the acquirer than expected. When the comovement of the acquirer with the market is greater than comovement of the target with the market ($\beta_A > \beta_T$), we expect the actual merged beta to be greater than the expected beta ($\beta_M > E(\beta)$). Hence, implying that the acquiring firm exhibits undue influence (relative to its market capitalization) on the comovement of the merged firm. Likewise, we expect the actual merged beta to be lower than the expected beta ($E(\beta) > \beta_M$) when the beta of the target is greater than the beta of the acquirer.

Figure 2 presents our pre merger betas (acquirer and target) and our post merger expected and observed beta. Panel A considers deals for which $\beta_A > \beta_T$ while Panel B considers deals for which $\beta_T > \beta_A$.

Insert Figure 2 here

Examining Panel A, it is evident that the actual merged beta is greater than the expected beta indicating excess comovement with the acquirer. Turning to Panel B, we see that the actual merged beta is slightly below the expected beta.

Table 2 compares the actual to the expected betas in our overall sample, split according to whether β_A is higher or lower than β_T, and tests whether the excess comovement is significant.

Insert Table 2 here

When $\beta_A > \beta_T$, the expected beta is 1.09 compared to the actual merged beta of 1.18. The difference between the actual and expected merged beta ($\beta_M - E(\beta)$, excess comovement) is statistically significant at the one percent level and represents a shift towards the acquirer's beta of 8.26 percent relative to the mean expected beta. This understates the effect on target betas since targets represent on average less than half of the merged firm.

To evaluate the economic impact on target betas, we calculate an implied target beta based on our estimates. We replace for $E(\beta)$ in $\beta_M = E(\beta)$ by using equation (1) and rearrange to obtain an expression for the implied target beta,

$$\beta_T^{\text{Imp}} = \frac{MV_A + (1-\lambda)MV_T}{(1-\lambda)MV_T}\widehat{\beta}_M - \frac{MV_A}{(1-\lambda)MV_T}\widehat{\beta}_A \tag{2}$$

Using our estimates $\widehat{\beta}_A$, $\widehat{\beta}_M$ we calculate an implied target beta for each transaction. The implied target beta ($\beta_T^{\text{Imp}} = 1.05$) is on average 34.2 percent larger than the pre merger estimated target beta ($\beta_T = 0.78$) when $\beta_A > \beta_T$.

Turning to the deals in which $\beta_T > \beta_A$, we see that the excess comovement is negative (-0.01) which is in line with our prediction. However, the difference is not economically or statistically significant. One potential explanation for this finding is that in order to observe excess comovement we require that target investors sell their shares. Therefore, splitting our results according to method of payment (see next section) provides for a more powerful test.

3.1.1 Method of Payment

If equity comovement is determined by the firm's investors, then the greater the fraction of target shareholders that exit following the merger, the greater the excess comovement (Brealey et al.) with the acquirer. In mergers that are paid only with cash, all target shareholders exit whereas in stock-for-stock mergers no target shareholder has to exit. Therefore, we expect the excess comovement with the acquirer to be significantly larger in cash mergers

than in stock mergers. Figure 3 presents pre merger and post merger betas of our cash deals according to whether $\beta_A > \beta_T$ (Panel A) or $\beta_T > \beta_A$ (Panel B).

Insert Figure 3 here

Both panels of Figure 3 are indicative of the post merger comovement having shifted significantly towards the comovement of the acquirer.

Panel A of Table 3 presents univariate analysis of pre and post merger betas of cash deals.

Insert Table 3 here

When the comovement of the acquirer is greater than the comovement of the target, the expected beta is 0.97 while the actual beta is 1.18 implying that the tilt towards the acquiring firm is 21.65 percent relative to the expected beta. Additionally, this difference is statistically significant at the one percent level. Further, the implied target beta calculated using equation (2) is 46.1 percent larger than the pre merger estimated target beta. Turning to the deals where $\beta_T > \beta_A$, we find an expected beta of 0.93 whereas the actual post merger beta is 0.84, the difference of -0.09 represents a -9.68 percent deviation from the expected merged beta. The implied target beta is now 27.5 percent lower than the pre merger beta. This difference is statistically significant at the 5 percent level.

Panel B of Table 3 presents our results for pure stock transactions. It is striking that irrespective of whether the target beta is higher or lower than the acquirer beta, the difference between the actual and expected post merger beta is never statistically nor economically significant.

3.1.2 Index Inclusion

Barberis, Shleifer and Wurgler (2005) document that the comovement with the S&P 500 increases after the inclusion into the S&P 500 and Vijh (1994) documents an increase in comovement with the CRSP value-weighted index following inclusion to the S&P 500. Given that investors have preferences and mandates to invest in particular stocks, some investors will be forced to liquidate their holdings of a company once it is included into the S&P 500 (e.g., small cap funds). This implies a greater degree of exit following inclusion and therefore we would predict greater excess comovement with the acquirer for those targets that are acquired by a S&P 500 firm. In this section, we examine the excess comovement of S&P 500 included targets and verify that our previous results are not driven by index inclusion. Index composition data is obtained from COMPUSTAT. In our sample we have 61 targets that are included into the index as a result of the merger.

Insert Table 4

In Panel *A* of Table 4 we consider those deals in which the target is included into the S&P 500.[6] Considering those deals for which $\beta_A > \beta_T$ we find a large shift towards the comovement of the acquirer. When considering implied target betas, the mean (median) firm experiences an increase in beta of 23.8 (38.8) percent. Unfortunately, the difference is not statistically significant, perhaps due to the low sample size. For the deals in which $\beta_A < \beta_T$, the shift towards the acquirer's comovement is larger. The mean (median) excess comovement $(\beta_M - E(\beta))$ is -0.15 (-0.13) which equals a deviation of 14.56 (12.04) percent. The effect in terms of implied target betas is larger, the mean (median) target firm experiences an decrease

[6]There are very few deals in which the acquirer is included into the S&P 500 as a result of the merger (33). However, for these deals we would expect excess comovement to be smaller since there will be forced exit on the side of the acquirer.

in beta of 46.6 (40.1) percent. These differences are statistically significant at the one percent level. Overall, the results of this panel are consistent with the findings of Barberis et al. that document a S&P 500 "habitat." Put differently, the excess comovement seems to be larger when the target is included in the index as a result of the merger.

To illustrate that our results are not driven by index inclusion, in Table 4 Panel B we consider those cash deals (where we predict and document the strongest effect) in which neither the acquirer nor the target experience a change in inclusion status from the start of the pre to the end of the post merger estimation windows. In the $\beta_A > \beta_T$ case, we find a large positive and statistically significant excess comovement while when $\beta_T > \beta_A$, it is negative and statistically significant. Hence, our results are qualitatively unchanged after removing index inclusions.

In summary, Table 4 documents that we observe an index inclusion effect consistent with previous work, but that this effect cannot explain our findings.

3.1.3 Industry

Investors often have a preference over what industry they invest in (e.g., Barberis and Shleifer, 2003). This implies that we expect to have a greater fraction of target shareholders exiting when mergers are across industries (e.g., industry specific mutual funds) rather than within industry. Hence, we predict the excess comovement towards the acquirer to be greater in across industry mergers than in intra industry mergers.

Table 5 splits mergers into those in which the acquirer and target have the same SIC code and those in which the SIC code of the target and the acquirer differs.[7]

Insert Table 5 here

[7]We have also used the S&P sector classification as our industry measure. The results are qualitatively unaltered, but with a significantly smaller sample of across industry mergers.

Panel A of Table 5 considers across industry mergers. When $\beta_A > \beta_T$ the excess comovement with the acquirer beta is 11.71 percent relative to the expected beta. Similarly, the economic effect in terms of implied betas is large and statistically significant at the one percent level. The excess comovement is negative when we consider $\beta_T > \beta_A$, but economically and statistically insignificant.

In Panel B, we do not find any evidence of excess comovement for within industry mergers, which is consistent our prediction.

3.1.4 Geography

There is a significant amount of evidence documenting that investors have a strong preference for local stocks (Coval and Moskowitz, 1999, and Huberman, 2001). In terms of geography and comovement, Pirinsky and Wang (2006) document that comovement with local stocks alters following changes in the location of firm headquarters.

Given the strong preference for local stocks, we expect greater target shareholder exit in across state mergers and therefore greater excess comovement with the acquirer. To test this, we classify mergers according to whether the headquarters (SDC) of the two merging firms are located in the same state.

Insert Table 6 here

Panel A of Table 6 considers mergers across state borders. For mergers in which $\beta_A > \beta_T$, we find that the tilt towards the acquirer beta is statistically significant at the one percent level. We consider same state mergers in Panel B and find no significant tilt towards the acquirer. Although our geography results are weaker than our results on industry and method of payment, they are indicative of excess comovement being greater for across state mergers.

3.1.5 Leverage

We follow Ghosh and Jain (2000) in computing the change in leverage due to the merger. The leverage ratio is the fiscal year-end ratio of debt to total firm value. We measure debt as the book value of long-term debt added to the debt in current liabilities as reported by Compustat. Total firm value is the book value of debt added to the market value of equity. To facilitate comparison of pre and post merger leverage we construct a hypothetical merged firm prior to announcement by pooling the balance sheet of the target and acquirer.

Figure 4 describes the leverage level from three years before the announcement to three years after the completion to cover the beta estimation windows.

Insert Figure 4 here

Panel A considers the leverage of our entire sample while Panel B considers only cash deals. The results parallel those of Ghosh and Jain, we find that leverage increases as a result of the merger. In Panel A the leverage increases by roughly seven percentage points from three years before the announcement to three years after completion. If we consider the time period from one year prior to the announcement to one year after completion, similar to Ghosh and Jain, we find that leverage increases by seven percent.

In both Panels, A and B, we have split our sample according to whether the beta of the target is higher or lower than the beta of the acquirer. The leverage pattern is strikingly similar irrespective of the relative riskiness of the acquirer and the target. In both cases the leverage increases due to the merger. Since Figure 4 documents that when the beta of the acquirer is smaller than the beta of the target, this leverage increase predicts a higher than expected post merger beta. However, in this case, Table 3 Panel A documents that the post merger beta is in fact lower than expected, indicating that leverage cannot explain our results.

Further, to make sure that leverage is not driving our results, in Table 7 we have split our cash deals (where we predict and document the strongest effect) according to whether they have above (Panel A) or below (Panel B) median change in leverage.

Insert Table 7 here

Table 7 provides different pieces of evidence to suggest that the excess comovement is not due to leverage. Firstly, our transactions in Panel A experience an insignificant average (median) increase in leverage of 2.23 (2.34) percent. Nonetheless, the post merger beta is higher than expected when the beta of the acquirer is larger than the beta of the target ($\beta_A > \beta_T$). Hence we document the effect in the absence of a leverage increase. The difference between the actual and the expected post merger beta is economically significant, however the reduction in power implies that we cannot reject the null. Secondly, in Panel B our transactions experience an average (median) increase in leverage of 34.30 (31.76) percent. For deals in which the beta of the target is larger than the beta of the acquirer (i.e., those deals where leverage increases are predicted to reduce excess comovement with the acquirer) we find a difference between the post merger beta and the expected beta of -0.09. This implies that the excess comovement is still economically significant even though leverage increases substantially. Even though the above two findings suggest that leverage does not explain our excess comovement, it is clear from Table 7 that leverage does influence estimated betas. Consider when the beta of the acquirer is larger than the beta of the target, going from Panel A to Panel B implies an increase in the point estimate of the excess comovement ($\beta_M - E(\beta)$) from 0.12 to 0.25. On the other hand, when the beta of the acquirer is lower than the beta of the target, going from Panel A to Panel B decreases the median excess comovement from -0.11 to -0.05 indicating that leverage increases are associated with increases in beta. In general, our results are weaker when the beta of the target is greater than the beta of the acquirer which can be justified by the observed leverage increase.

The results of this section are indicative of excess comovement being independent of leverage, which our regression analysis below provides further evidence of.

3.2. Regression Analysis

3.2.1 Deal Characteristics and Excess Comovement

In this section, we pool all transactions and use regression analysis to document the existence of excess comovement while controlling for deal specific factors. To examine whether the post merger beta is closer to the acquirer, we use as dependent variable excess comovement ($\beta_M - E(\beta)$, equation (1)). To capture the asymmetric prediction of the tilt being positive when $\beta_A > \beta_T$ (see Figure 3, Panel A) and negative when $\beta_T > \beta_A$ (see Figure 3, Panel B) we consider as explanatory variable the conditional beta dummy ($\beta_A|\beta_T$) which takes the value of $1(-1)$ if the beta of the acquirer is larger (smaller) than the beta of the target. This implies that we always expect a positive relation between $\beta_A|\beta_T$ and our dependent variable ($\beta_M - E(\beta)$). We estimate the following regression:

$$\beta_M - E(\beta) = \alpha + b_1(\beta_A|\beta_T) + \mathbf{\Gamma'W} + \varepsilon \tag{3}$$

where $\mathbf{W}$ is vector of control variables, $\mathbf{\Gamma}$ is a vector of coefficients and ε is an error term.

Insert Table 8 here

Table 8 contains the results from our regression analysis. In the first specification, we estimate equation (3) without control variables. Our main variable of interest, $\beta_A|\beta_T$, is positively and significantly related to excess comovement.

Specification (2) introduces our control variables. To make sure our results are not driven by leverage we introduce as a control variable the absolute change in leverage (defined in section A.5 of the Empirical Findings). It is comforting that the coefficient on leverage is positive and significant, indicating that post merger betas are increased as a result of the leverage added in the merger. Additionally, we control for the synergies associated with the merger and the relative market capitalization of the target. Finally, we control for changes in comovement due to index inclusions with our dummy variable *Index Inclusion*.[8]

The effect of the conditional beta dummy ($\beta_A|\beta_T$) is economically significant, going from deals where $\beta_A < \beta_T$ ($\beta_A|\beta_T$ takes the value -1) to deals where $\beta_A > \beta_T$ results in an increase in excess comovement of 0.09 (specification (2)). This represents a 225 percent change compared to the average excess comovement of the full sample (0.04). Put in terms of betas, our results imply that the post merger beta of the firm increases by 0.09 when $\beta_A|\beta_T$ goes from -1 to 1. This represents a 8.75 percent change compared to the average target beta in our sample.[9]

For those deals in which we expect particularly large excess comovement, cash (specification (3)), target index inclusion (specification (5)), across industry mergers (specification (6)) and across state mergers (specification (8)), the conditional beta dummy is at least statistically significant at the five percent level. However, we find no evidence of excess comovement in stock deals (specification (4)), same industry (specification (7)) and same state transactions (specification (9)). As expected, there is substantial variation in economic impact across deal characteristics. For example, the economic impact of cash transactions and deals in which the target is included in the S&P 500 is three times the economic impact

[8]In our sample we have 66 targets and 37 aquirers that change their S&P 500 status (inclusions and deletions) from the start of the pre merger to end of the post merger estimation period. Since our goal here is to make sure that our regression results are not driven by changes in S&P 500 status, we control for any change in status for either acquirers or targets.

[9]In this regression, the estimated economic impact probably understates the true impact on target betas since targets, on average, represent 35 percent of the market capitalization of the merged firm.

of the full sample. Overall, the results of Table 8 demonstrate the existence of excess comovement while controlling for leverage and transaction synergies. The next section formally tests whether there are differences in excess comovement across deal characteristics.

3.2.2 Shareholder Exit and Excess Comovement

In this section, we verify that deals that should be associated with greater shareholder exit also experience greater excess comovement. To capture that, we expect the tilt towards the acquirer's beta to be larger in cash transactions (due to the complete exit of target shareholders), we interact our dummy variable *Cash* with the conditional beta dummy ($\beta_A|\beta_T$). Our prediction is that *Cash* transactions are associated with a greater excess comovement than *Stock* deals. To test this, we keep deals financed with 100 percent stock as our base category and introduce interacted (with $\beta_A|\beta_T$) dummy variables for all other categories (*Cash*, *Mixed* and *Other*). Thus, specification (3) in Table 9 estimates the following regression,

$$\beta_M - E(\beta) = \alpha + b_2(\beta_A|\beta_T \times Cash) + b_3(\beta_A|\beta_T \times Mixed) + b_4(\beta_A|\beta_T \times Other) + \mathbf{\Gamma'W} + \varepsilon$$

As expected, we find that the excess comovement with the acquirer is statistically significantly greater for cash transactions than for stock transactions. In terms of economic magnitude, cash transactions are associated with a 450 percent greater excess comovement than stock transactions.

Insert Table 9

In specification (4), we consider whether the excess comovement with the acquirer is larger in cases where the target is included in the S&P 500 as a result of the merger.

To do so, we create the dummy variable, *Target Inclusion.* Specifically, this dummy variable takes the value of 1 if the acquirer is included in the S&P 500 prior to the announcement whereas the target is not and the merged firm is not excluded from the S&P 500 during our post estimation window. In the regression, we interact the dummy variable with the conditional beta dummy ($\beta_A|\beta_T$). The point estimate of the coefficient is positive and economically as well as statistically (one sided test at the ten percent level) significant. Thus, suggesting that index inclusion is associated with significant investor exit which results in a larger tilt towards the comovement of the acquirer. The economic magnitude is large and comparable to that of cash deals.

Similarly, we also consider whether the degree of excess comovement varies with other factors such as geography and industry that investors have a clear preference for. To do so, we create two dummy variables, *Different SIC* and *Different State,* that take the value 1 if target and acquirer have different SIC codes and are headquartered in different states respectively and 0 otherwise. Like before, we interact the dummy variables with the conditional beta dummy ($\beta_A|\beta_T$). In specification (5), we examine whether mergers across industries are associated with a tilt towards the acquirer. The coefficient on our interaction variable $\beta_A|\beta_T \times$ *Different SIC* is positive and significant in both economic and statistical terms. Finally, in specification (6), we consider whether deals in which the target and acquirer are located in different states are associated with larger post merger beta differentials. The coefficient on our interaction variable is positive and statistically significant at the ten percent level. The results of Table 9 suggest that shareholder exit significantly impacts excess comovement.

4. Robustness

One concern raised by Brealey et al. (2010) is that the merger transforms the targets assets to become more like the assets of the acquirer. However, for this transformation to explain our findings it must be that the transformation is more rapid for cash transactions and deals in which the target is included in the S&P 500 index. Furthermore, this transformation has to be rather rapid since we estimate our post merger beta over 100 weeks following completion. Additionally, we follow Brealey et al. and document the progression of comovement post completion. If asset transformation is driving our results we would expect that the unobserver target beta tends towards the acquirer beta over time. This has several implications. First, the beta of the merged firm should be changing as the assets are being transformed. Second, when the beta of the acquirer is greater than the beta of the target ($\beta_A > \beta_T$), as the beta of the target converges to that of the acquirer the beta of the merged firm should increase. Third, when $\beta_T > \beta_A$ the beta of the merged firm should decrease as the transformation progresses. To test these predictions we have estimated the post merger beta over 100 weeks starting in eight consecutive quarters following completion.

Insert Figure 5 here

Panel A of Figure 5 considers the progression of the post merger beta for cash deals in which $\beta_A > \beta_T$. Contrary to the asset transformation hypothesis we do not find an increase in the post merger beta over time. The beta at completion is 1.17 and the last estimated beta is 1.08.[10] The difference between the two is not statistically significant. In Panel B of Figure 5 we consider cash deals for which $\beta_T > \beta_A$. There is no discernible trend in beta

[10]The corresponding post merger beta estimated in Table 3 is 1.18 (compared to 1.17 in this section). To consider the progression in comovement we require the firm to be present in CRSP four years following the completion and this results in a loss of three observations compared to Table 3.

over time, the beta at completion is 0.82 and the last estimated beta is 0.85. The difference is neither statistically nor economically significant.[11]

5. Conclusion

Previous studies (Chan, et al., 2003, and Brealey et al., 2010) document excess comovement in international equity markets. These papers study events in which the shareholder base of the firm is expected to change (listings and cross-border mergers) and relate this to changes in comovement. Thereby providing evidence of international segmentation and suggesting that stocks are priced on country level rather than internationally (see Karolyi and Stulz, 2003 for a review of the literature).

This paper provides evidence of excess comovement in US mergers and thereby of segmentation on a national level. We do this by comparing a post merger beta to an expected post merger beta based on the pre merger comovement of the target and the acquirer. We find that the post merger comovement is shifted towards the comovement of the acquirer.

In a similar vein, Barberis et al. (2005) document segmentation on a national level by showing that stocks that are included into the S&P 500 experience an increase in comovement with the S&P 500. They argue that there are investor habitats and therefore index inclusion is associated with investor entry and exit. When we consider those target firms that are included into the S&P 500, we find evidence suggesting that the shift towards the acquirer is larger corroborating the existence of a S&P 500 habitat. Additionally, we verify that our results remain qualitatively unchanged even in the absence of index inclusion. Relying on mergers for identification rather than index inclusion has the advantage that we can consider segmentation along other dimensions over which investors may show a preference for, such as industry, geographic location (e.g., Pirinsky and Wang, 2006) and index membership.

[11] Using the overall sample we have confirmed that asset transformation is not driving our results.

Identifying excess comovement relies on two assumptions; first that there is entry or exit of investors associated with the event and second that nothing else is altered as a result of the event. Arguably, firms may undergo significant changes as a result of a merger (e.g., leverage may increase) and therefore, it is important that we are careful in considering alternative stories. However, for any alternative story to explain our results it must be the case that the explanation generates asymmetric predictions with respect to the relative riskiness of the acquirer and the target. That is, the story has to jointly explain why the post merger beta is greater than expected when the comovement with the market of the acquirer is greater than that of the target and why the post merger beta is smaller than expected when the target is riskier than the acquirer. For example in terms of leverage, it must increase in one set of transactions and decrease for the complement. In particular, we control for changes in leverage and synergies in our analysis.

Given the mounting evidence that markets are segmented (both internationally and nationally), this suggests that care should be taken when estimating betas in situations in which a significant proportion of the investor base has been altered.

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Figure 1
Timeline

This figure depicts the timeline of our research design. During the pre run-up period, which lasts for 100 weeks and ends eight weeks prior to the merger announcement, we estimate the betas for the acquiror (β_A) and the target (β_T). $E(\beta)$ is the market value weighted average of these betas, adjusted for a possible toehold. The run-up period, covering the eight weeks prior to announcement, is excluded from the estimation period due to the possibility of informed trading. The post merger period lasts for 100 weeks after completion. In this period we estimate the beta of the merged firm, β_M.

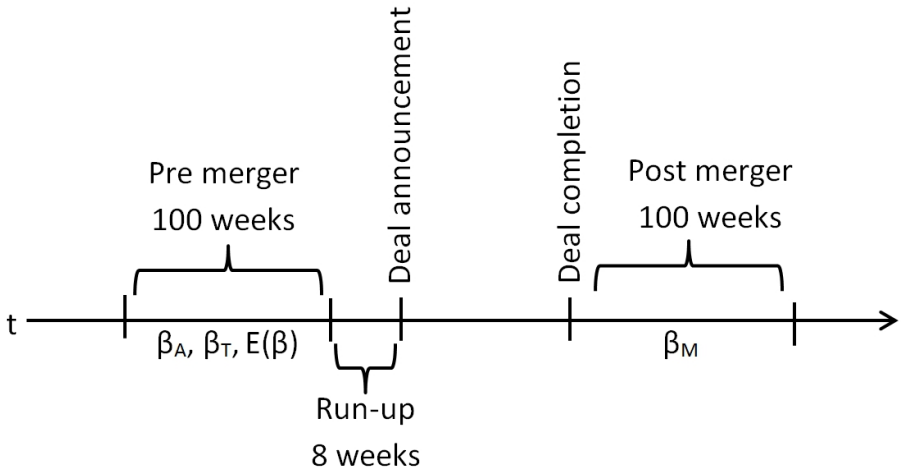
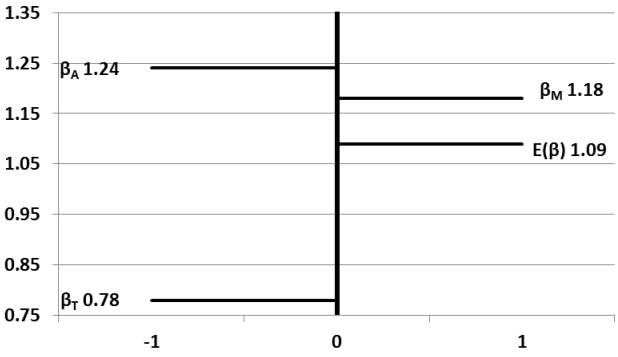


Figure 2
Comovement changes in mergers

These figures illustrate changes in comovement as a result of the merger. Panel A illustrates the deals for which $\beta_A > \beta_T$ and Panel B the deals for which $\beta_A < \beta_T$. The horizontal axis represents the timing of the merger: (-1) is the pre and (1) the post merger period. β_A and β_T are the pre merger betas of the acquirer and target respectively. $E(\beta)$ is the expected beta of the merged firm, calculated as the market value weighted average of β_A and β_T, adjusted for a possible toehold. Finally, β_M is the beta of the merged firm after completion.

(a) Panel A: $\beta_A > \beta_T$



(b) Panel B: $\beta_T > \beta_A$

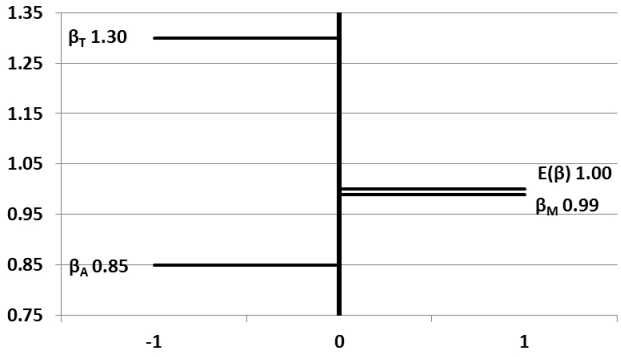
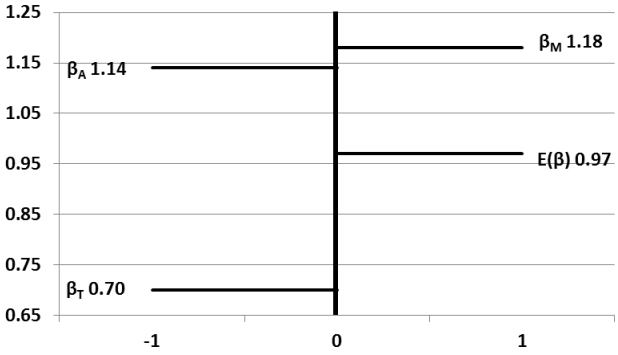


Figure 3
Comovement changes in cash mergers

These figures illustrate changes in comovement as a result of the merger for cash deals only. Panel A illustrates the deals for which $\beta_A > \beta_T$ and Panel B the deals for which $\beta_A < \beta_T$. The horizontal axis represents the timing of the merger: (-1) is the pre and (1) the post merger period. β_A and β_T are the pre merger betas of the acquirer and target respectively. $E(\beta)$ is the expected beta of the merged firm, calculated as the market value weighted average of β_A and β_T, adjusted for a possible toehold. Finally, β_M is the beta of the merged firm after completion.

(a) Panel A: $\beta_A > \beta_T$



(b) Panel B: $\beta_T > \beta_A$

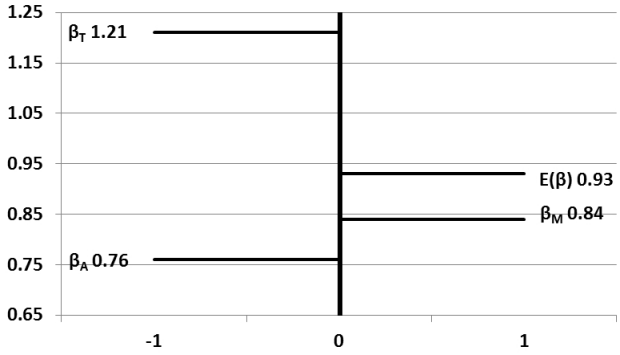
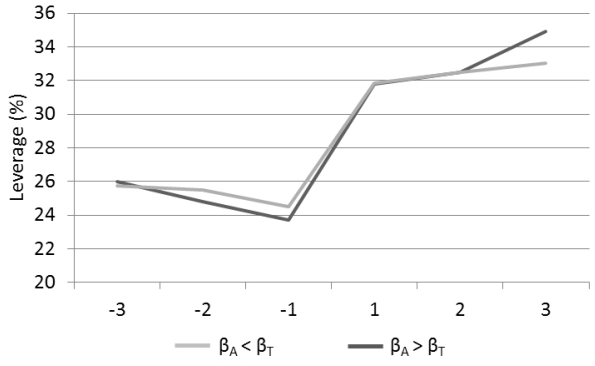


Figure 4
Average change in leverage around mergers

This figure illustrates the average change in leverage around mergers. Panel A contains the data of the full sample while Panel B contains cash deals only. Furthermore, the data is split according to whether $\beta_A > \beta_T$ or $\beta_A < \beta_T$. Leverage is defined as the end-of-year ratio of the sum of the acquirer's and target's book value of debt to the sum of the acquirer's and target's total market value. Total market value is defined as book value of debt plus the market value of equity. Book value of debt is defined as the sum of long-term debt (Compustat-Item dltt) plus debt in current liabilities (Compustat-Item dlc). The leverage levels are shown for the three years prior to announcement and the three years after completion.

(a) Panel A: Full Sample



(b) Panel B: Cash Transactions

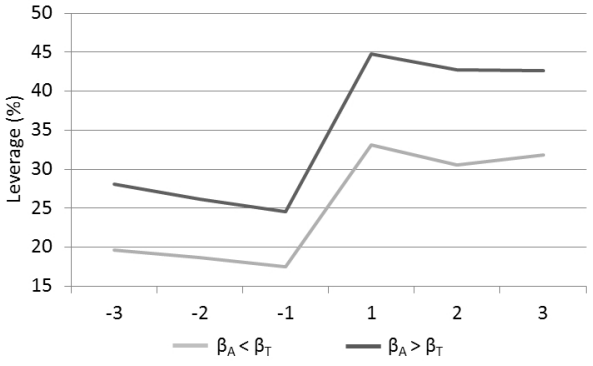
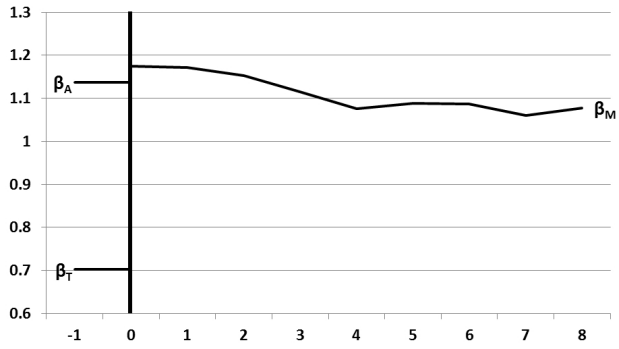


Figure 5
Progression of the merged firm's beta

This figure depicts the progression of the merged firm's beta over time. Panel A illustrates the deals for which $\beta_A > \beta_T$ and Panel B the deals for which $\beta_A < \beta_T$. The horizontal axis represents the timing of the merger: (-1) is the pre merger period, (0) the completion period and positive numbers are quarters after completion (one to eight). During the pre run-up period, which lasts for 100 weeks and ends eight weeks prior to the merger announcement, we estimate the betas for the acquirer (β_A) and the target (β_T). The beta of the merged firm, β_M, is estimated for the first time at completion using 100 weeks of data. In each of the consecutive eight quarters, the beta of the merged firm is estimated anew using 100 weeks of data.

(a) Panel A: $\beta_A > \beta_T$



(b) Panel B: $\beta_T > \beta_A$

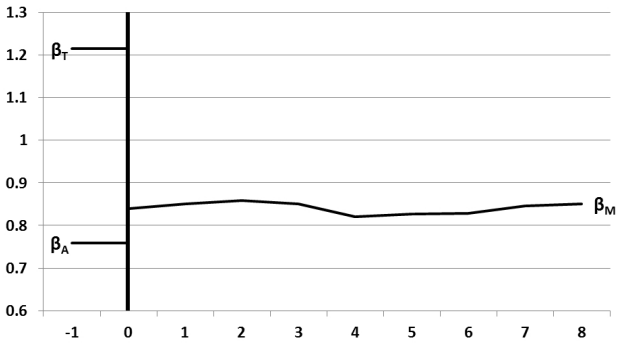


Table 1
Summary statistics of our main variables

We present descriptive statistics for our main variables. *MV Target* and *MV Acquirer* is the market value in Mio. USD of the target and the acquirer firm eight weeks prior to deal announcement. *Target Weight* is the ratio of MV Target to the combined market value, MV Target plus MV Acquirer, adjusted for a possible toehold. *Leverage ex-ante* is the end-of-year ratio of the sum of the acquirer's and target's book value of debt to the combined total market value for the year prior to announcement. Total market value is defined as book value of debt plus market value of equity. Book value of debt is calculated as the sum of long-term debt (Compustat-Item dltt) plus debt in current liabilities (Compustat-Item dlc). *Leverage ex-post* is calculated analogously for the year after deal completion. *Synergies* is the ratio of the combined, market adjusted abnormal value (target and acquirer) created over a six week window around the merger announcement (three weeks before and three weeks after) relative to the combined market value of the target and acquirer eight weeks prior to the merger announcement. Synergies are winsorized at the one and 99 percent level. λ is the toehold the acquirer owns at deal announcement. β's are estimated using weekly data over a 100 weeks estimation period. β_A is the acquirer's and β_T the target's beta based on an estimation window ending eight weeks prior to deal announcement. E(β) is the expected beta of the merged firm, calculated as market value (MV Target and MV Acquirer) weighted average of β_A and β_T, adjusted for possible toeholds. β_M is the beta of the merged firm calculated after deal completion. All β's are winsorized at the one and 99 percent level.

Variable	Mean	Median	Std. Dev.	Min	Max	N
MV Target (Mio.)	2'360	7'117	461	28	81'900	712
MV Acquirer (Mio.)	4'928	17'000	987	22	230'000	712
Target Weight	34.47%	15.74%	32.49%	2.74%	79.77%	712
Leverage ex-ante	23.91%	18.02%	21.05%	0.00%	83.66%	693
Leverage ex-post	31.74%	23.38%	28.27%	0.00%	94.99%	703
Synergies	6.23%	16.76%	5.08%	-38.59%	68.71%	712
λ	2.95%	12.87%	0.00%	0.00%	95.35%	712
β_A	1.05	0.60	0.98	-0.27	3.01	712
β_T	1.03	0.64	0.98	-0.59	3.16	712
E(β)	1.05	0.54	1.00	-0.31	2.90	712
β_M	1.09	0.57	1.03	-0.44	2.78	712

Table 2
Univariate Results for the full sample

We present univariate results for the full sample according to whether $\beta_A > \beta_T$ (columns 2 and 3) or $\beta_A < \beta_T$ (columns 4 and 5). β_A and β_T are the pre merger betas of the acquirer and the target. $E(\beta)$ is the expected merged beta, calculated as the market value weighted average of the target and the acquirer beta. Weights are adjusted for a possible toehold. β_M is the actual merged beta and $\beta_M - E(\beta)$ is *Excess Comovement*. We report mean and median test statistics (in brackets) for Excess Comovement being positive ($\beta_M - E(\beta) > 0$) when $\beta_A > \beta_T$ and negative ($\beta_M - E(\beta) < 0$) when $\beta_A < \beta_T$. β_T^{Imp} is the implicit value of the target beta which would be required for $\beta_M = E(\beta)$ to hold. $\beta_T^{Imp} - \beta_T$ is the difference between the implicit and the observed target beta. Testing procedure and statistics are as above. *Change* is the difference between β_T^{Imp} and β_T in percent. All betas are winsorized at the one and 99 percent level. Test statistics are reported for one sided tests with the significance levels of ten percent(*), five percent(**) and one percent(***).

Overall Sample				
	$\beta_A > \beta_T$		$\beta_T > \beta_A$	
	Mean	Median	Mean	Median
β_A	1.24	1.19	0.85	0.80
β_T	0.78	0.75	1.30	1.18
$E(\beta)$	1.09	1.04	1.00	0.96
β_M	1.18	1.11	0.99	0.94
$\beta_M - E(\beta)$	0.09***	0.06***	-0.01	-0.05
	(3.08)	(2.47)	(-0.45)	(-0.71)
β_T^{Imp}	1.05	0.93	1.26	1.10
$\beta_T^{Imp} - \beta_T$	0.27**	0.18**	-0.04	-0.08
	(2.32)	(1.95)	(-0.43)	(-0.66)
Change	34.2%	23.4%	-3.2%	-7.2%
N	372	372	340	340

Table 3

Univariate results according to method of payment

We present univariate results for different methods of payment. Panel A restricts the full sample to cash deals only while Panel B considers pure stock deals. We measure Excess Comovement according to whether $\beta_A > \beta_T$ (columns 2, 3, 6 and 7) or $\beta_A < \beta_T$ (columns 4, 5, 8 and 9). β_A and β_T are the pre merger betas of the acquirer and the target. $E(\beta)$ is the expected merged beta, calculated as the market value weighted average of the target and the acquirer beta. Weights are adjusted for a possible toehold. β_M is the actual merged beta and $\beta_M - E(\beta)$ is *Excess Comovement*. We report mean and median test statistics (in brackets) for Excess Comovement being positive ($\beta_M - E(\beta) > 0$) when $\beta_A > \beta_T$ and negative ($\beta_M - E(\beta) < 0$) when $\beta_A < \beta_T$. β_T^{Imp} is the implicit value of the target beta which would be required for $\beta_M = E(\beta)$ to hold. $\beta_T^{Imp} - \beta_T$ is the difference between the implicit and the observed target beta. Testing procedure and statistics are as above. *Change* is the difference between β_T^{Imp} and β_T in percent. All betas are winsorized at the one and 99 percent level. Test statistics are reported for one sided tests with the significance levels of ten percent(*), five percent(**) and one percent(***).

	Panel A: Cash Deals				Panel B: Stock Deals			
	$\beta_A > \beta_T$		$\beta_T > \beta_A$		$\beta_A > \beta_T$		$\beta_T > \beta_A$	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
β_A	1.14	1.14	0.76	0.73	1.36	1.26	0.96	0.89
β_T	0.70	0.69	1.21	1.10	0.86	0.83	1.43	1.22
$E(\beta)$	0.97	0.99	0.93	0.87	1.20	1.12	1.11	1.02
β_M	1.18	1.12	0.84	0.83	1.23	1.18	1.11	1.03
$\beta_M - E(\beta)$	0.20***	0.14***	-0.09**	-0.11**	0.03	-0.05	0.00	-0.04
	(2.97)	(2.42)	(-1.70)	(-1.77)	(0.70)	(0.24)	(-0.06)	(-0.18)
β_T^{Imp}	1.03	1.07	0.88	0.93	0.96	0.66	1.39	1.29
$\beta_T^{Imp} - \beta_T$	0.32*	0.38**	-0.33**	-0.17**	0.11	-0.17	-0.04	0.07
	(1.41)	(1.88)	(-1.94)	(-1.75)	(0.56)	(-0.05)	(-0.20)	(0.12)
Change	46.1%	54.5%	-27.5%	-15.2%	12.7%	-20.2%	-2.5%	5.8%
N	66	66	58	58	148	148	115	115

Table 4

Univariate results according to index inclusion

We present univariate results for index inclusion. Panel A restricts the full sample to deals where the target has become part of the S&P 500 Index as a result of the transaction. Panel B restricts the full sample to only cash deals that are not affected by any change in S&P 500 listing status. We measure Excess Comovement according to whether $\beta_A > \beta_T$ (columns 2, 3, 6 and 7) or $\beta_A < \beta_T$ (columns 4, 5, 8 and 9). β_A and β_T are the pre merger betas of the acquirer and the target. $E(\beta)$ is the expected merged beta, calculated as the market value weighted average of the target and the acquirer beta. Weights are adjusted for a possible toehold. β_M is the actual merged beta and $\beta_M - E(\beta)$ is *Excess Comovement*. We report mean and median test statistics (in brackets) for Excess Comovement being positive ($\beta_M - E(\beta) > 0$) when $\beta_A > \beta_T$ and negative ($\beta_M - E(\beta) < 0$) when $\beta_A < \beta_T$. β_T^{Imp} is the implicit value of the target beta which would be required for $\beta_M = E(\beta)$ to hold. $\beta_T^{Imp} - \beta_T$ is the difference between the implicit and the observed target beta. Testing procedure and statistics are as above. *Change* is the difference between β_T^{Imp} and β_T in percent. All betas are winsorized at the one and 99 percent level. Test statistics are reported for one sided tests with the significance levels of ten percent(*), five percent(**) and one percent(***).

	Panel A: Target Index Incl.				Panel B: Cash Deals w/o Index Incl.			
	$\beta_A > \beta_T$		$\beta_T > \beta_A$		$\beta_A > \beta_T$		$\beta_T > \beta_A$	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
β_A	0.98	0.87	0.92	0.92	1.14	1.16	0.75	0.73
β_T	0.68	0.60	1.32	1.32	0.68	0.65	1.21	1.08
$E(\beta)$	0.89	0.80	1.03	1.08	0.96	1.01	0.93	0.87
β_M	1.00	0.86	0.87	0.93	1.17	1.13	0.83	0.82
$\beta_M - E(\beta)$	0.11	0.13	-0.15***	-0.13***	0.21***	0.16***	-0.09**	-0.11**
	(1.15)	(1.09)	(-2.54)	(-2.54)	(2.89)	(2.31)	(-1.69)	(-1.75)
β_T^{Imp}	0.84	0.84	0.71	0.79	1.09	1.02	0.89	0.98
$\beta_T^{Imp} - \beta_T$	0.16	0.23	-0.62**	-0.53***	0.41**	0.37**	-0.32**	-0.11*
	(0.41)	(0.58)	(-2.37)	(-2.56)	(1.71)	(1.83)	(-1.84)	(-1.65)
Change	23.8%	38.8%	-46.6%	-40.1%	59.7%	56.8%	-26.4%	-9.7%
N	32	32	29	29	57	57	52	52

Table 5

Univariate results according to industry

We present univariate results for intra versus inter industry mergers. Panel A restricts the full sample to across industry deals (different SIC) only while Panel B considers within industry deals (same SIC) exclusively. We measure Excess Comovement according to whether $\beta_A > \beta_T$ (columns 2, 3, 6 and 7) or $\beta_A < \beta_T$ (columns 4, 5, 8 and 9). β_A and β_T are the pre merger betas of the acquirer and the target. $E(\beta)$ is the expected merged beta, calculated as the market value weighted average of the target and the acquirer beta. Weights are adjusted for a possible toehold. β_M is the actual merged beta and $\beta_M - E(\beta)$ is *Excess Comovement*. We report mean and median test statistics (in brackets) for Excess Comovement being positive ($\beta_M - E(\beta) > 0$) when $\beta_A > \beta_T$ and negative ($\beta_M - E(\beta) < 0$) when $\beta_A < \beta_T$. β_T^{Imp} is the implicit value of the target beta which would be required for $\beta_M = E(\beta)$ to hold. $\beta_T^{Imp} - \beta_T$ is the difference between the implicit and the observed target beta. Testing procedure and statistics are as above. *Change* is the difference between β_T^{Imp} and β_T in percent. All betas are winsorized at the one and 99 percent level. Test statistics are reported for one sided tests with the significance levels of ten percent(*), five percent(**) and one percent(***).

	Panel A: Different SIC				Panel B: Same SIC			
	$\beta_A > \beta_T$		$\beta_T > \beta_A$		$\beta_A > \beta_T$		$\beta_T > \beta_A$	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
β_A	1.27	1.23	0.83	0.77	1.19	1.11	0.88	0.85
β_T	0.77	0.74	1.28	1.18	0.80	0.76	1.32	1.18
$E(\beta)$	1.11	1.07	0.98	0.90	1.06	1.00	1.03	1.02
β_M	1.23	1.15	0.97	0.93	1.09	1.06	1.02	0.95
$\beta_M - E(\beta)$	0.13***	0.11***	-0.02	-0.02	0.04	-0.04	0.00	-0.08
	(3.15)	(2.78)	(-0.51)	(-0.45)	(0.87)	(0.33)	(-0.11)	(-0.54)
β_T^{Imp}	1.17	0.97	1.22	1.15	0.87	0.84	1.30	1.09
$\beta_T^{Imp} - \beta_T$	0.40***	0.23**	-0.06	-0.03	0.07	0.08	-0.02	-0.09
	(2.49)	(2.26)	(-0.46)	(-0.32)	(0.43)	(0.19)	(-0.12)	(-0.65)
Change	51.9%	31.2%	-4.6%	-2.8%	8.5%	9.9%	-1.4%	-7.5%
N	224	224	197	197	148	148	143	143

Table 6

Univariate results according to geography

We present univariate results for within and across State mergers. Panel A restricts the full sample to across State deals (different State) only while Panel B considers within State deals (same State) exclusively. We measure Excess Comovement according to whether $\beta_A > \beta_T$ (columns 2, 3, 6 and 7) or $\beta_A < \beta_T$ (columns 4, 5, 8 and 9). β_A and β_T are the pre merger betas of the acquirer and the target. $E(\beta)$ is the expected merged beta, calculated as the market value weighted average of the target and the acquirer beta. Weights are adjusted for a possible toehold. β_M is the actual merged beta and $\beta_M - E(\beta)$ is *Excess Comovement*. We report mean and median test statistics (in brackets) for Excess Comovement being positive ($\beta_M - E(\beta) > 0$) when $\beta_A > \beta_T$ and negative ($\beta_M - E(\beta) < 0$) when $\beta_A < \beta_T$. β_T^{Imp} is the implicit value of the target beta which would be required for $\beta_M = E(\beta)$ to hold. $\beta_T^{Imp} - \beta_T$ is the difference between the implicit and the observed target beta. Testing procedure and statistics are as above. *Change* is the difference between β_T^{Imp} and β_T in percent. All betas are winsorized at the one and 99 percent level. Test statistics are reported for one sided tests with the significance levels of ten percent(*), five percent(**) and one percent(***).

	Panel A: Different State				Panel B: Same State			
	$\beta_A > \beta_T$		$\beta_T > \beta_A$		$\beta_A > \beta_T$		$\beta_T > \beta_A$	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
β_A	1.23	1.17	0.86	0.83	1.26	1.22	0.84	0.73
β_T	0.77	0.72	1.32	1.18	0.81	0.81	1.23	1.19
$E(\beta)$	1.08	1.01	1.01	0.96	1.10	1.09	0.99	0.87
β_M	1.18	1.12	0.99	0.95	1.17	1.09	1.00	0.93
$\beta_M - E(\beta)$	0.10***	0.07***	-0.02	-0.07	0.07	-0.03	0.02	0.04
	(2.90)	(2.47)	(-0.76)	(-1.01)	(1.09)	(0.51)	(0.31)	(0.30)
β_T^{Imp}	1.09	0.92	1.25	1.08	0.93	1.03	1.26	1.26
$\beta_T^{Imp} - \beta_T$	0.31***	0.20**	-0.07	-0.10	0.12	0.22	0.02	0.06
	(2.35)	(1.94)	(-0.55)	(-0.98)	(0.52)	(0.53)	(0.15)	(0.41)
Change	40.6%	27.9%	-5.0%	-8.1%	14.4%	27.0%	1.9%	5.1%
N	285	285	249	249	87	87	91	91

Table 7

Univariate results for changes in leverage

We present univariate results for different level of changes in leverage. We restrict the sample to cash deals only. Panel A contains all deals where the change in leverage is below the median (Q1) change. Panel B includes all deals where the change in leverage is above the median (Q2) change. The mean (median) change in leverage is reported in the second row of each panel. We measure Excess Comovement according to whether $\beta_A > \beta_T$ (columns 2, 3, 6 and 7) or $\beta_A < \beta_T$ (columns 4, 5, 8 and 9). β_A and β_T are the pre merger betas of the acquirer and the target. $E(\beta)$ is the expected merged beta, calculated as the market value weighted average of the target and the acquirer beta. Weights are adjusted for a possible toehold. β_M is the actual merged beta and $\beta_M - E(\beta)$ is *Excess Comovement*. We report mean and median test statistics (in brackets) for Excess Comovement being positive ($\beta_M - E(\beta) > 0$) when $\beta_A > \beta_T$ and negative ($\beta_M - E(\beta) < 0$) when $\beta_A < \beta_T$. β_T^{Imp} is the implicit value of the target beta which would be required for $\beta_M = E(\beta)$ to hold. $\beta_T^{Imp} - \beta_T$ is the difference between the implicit and the observed target beta. Testing procedure and statistics are as above. *Change* is the difference between β_T^{Imp} and β_T in percent. All betas are winsorized at the one and 99 percent level. Test statistics are reported for one sided tests with the significance levels of ten percent(*), five percent(**) and one percent(***).

	Panel A: Δ Leverage Q1 2.23% (2.34%)				Panel B: Δ Leverage Q2 34.70% (32.04%)			
	$\beta_A > \beta_T$		$\beta_T > \beta_A$		$\beta_A > \beta_T$		$\beta_T > \beta_A$	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
β_A	1.24	1.14	0.77	0.72	1.10	1.16	0.70	0.70
β_T	0.76	0.72	1.26	1.10	0.72	0.76	1.13	1.02
$E(\beta)$	1.10	1.01	0.93	0.89	0.92	0.97	0.90	0.83
β_M	1.22	1.17	0.84	0.84	1.17	1.11	0.80	0.80
$\beta_M - E(\beta)$	0.12	0.06	-0.09*	-0.11*	0.25***	0.16**	-0.09	-0.05
	(1.20)	(0.80)	(-1.33)	(-1.38)	(2.47)	(2.06)	(-1.06)	(-1.02)
β_T^{Imp}	0.86	0.85	0.75	0.81	1.06	1.26	0.98	0.98
$\beta_T^{Imp} - \beta_T$	0.10	0.39	-0.51**	-0.28**	0.34	0.49**	-0.15	-0.04
	(0.27)	(0.00)	(-1.99)	(-1.76)	(1.21)	(2.00)	(-0.94)	(-0.76)
Change	13.5%	19.0%	-40.3%	-25.7%	47.9%	64.5%	-13.3%	-4.1%
N	26	26	32	32	35	35	22	22

Table 8

Regression results for subsamples

We present the regression results for different subsets of the sample in this table. The dependent variable is the difference between the observed and the expected beta of the merged firm ($\beta_M - E(\beta)$). Each column is named after the selection criterion of the sample subset it covers. $\beta_A|\beta_T$ measures relative riskiness and is equal to (-1) for $\beta_A < \beta_T$ and 1 for $\beta_A > \beta_T$. $\beta_A|\beta_T \times Cash$, $\beta_A|\beta_T \times Mixed$ and $\beta_A|\beta_T \times Other$ are interaction variables of the dummy variables of cash, mixed and other deals with $\beta_A|\beta_T$. The base category in regression (3) are stock deals. $\beta_A|\beta_T \times Target\ \ Inclusion$ is an interaction variable of Target Index Inclusion, a dummy equal to one in case the Target is included in the S&P500 Index as a result of the merger, with $\beta_A|\beta_T$. $\beta_A|\beta_T \times Different\ \ SIC$ is an interaction variable of Different SIC, a dummy equal to one in case the SIC of the Acquiror and the Target are different, with $\beta_A|\beta_T$. $\beta_A|\beta_T \times DifferentState$ is an interaction variable of Different State, a dummy equal to one in case the State of the Acquiror and the the Target are different, with $\beta_A|\beta_T$. *Target Weight* is the market capitalization of the the target company, adjusted by toeholds, relative to the combined market capitalization of the target and the acquiror firm eight weeks prior to the merger announcement. *Change in Leverage* is the difference in end-of-year leverage of the combined balance sheet (target and acquiror) one year prior to the merger announcement to the merged firm end-of-year leverage one year after the completion of the deal. Leverage is measured as book value of debt to total market value which is defined as book value of debt plus market value of equity. Change in Leverage is winsorized at the one percent level. *Synergy Weight* is the ratio of the target's and acquiror's combined market adjusted abnormal value created over a three week window around the merger announcement relative to the combined market value eight weeks prior to the merger announcement. *Index Inclusion* is a control variable equal to one if either the acquiror or the target changed their S&P 500 listing status during the period we track the stock. All regressions control for a deal announcement year fixed-effect. t-statistics are calculated for a one sided test ($\beta_M - E(\beta) > 0$) with a robust variance estimator and reported in brackets with significance levels of ten percent(*), five percent(**) and one percent(***).

Dependent Variable:	$\beta_M - E(\beta)$								
	(1) Full	(2) Full	(3) Cash	(4) Stock	(5) Target Incl.	(6) Diff SIC	(7) Same SIC	(8) Diff State	(9) Same State
$\beta_A\|\beta_T$	0.052***	0.045**	0.136***	0.013	0.13**	0.065**	0.015	0.058**	0.023
	(2.56)	(2.18)	(2.82)	(0.40)	(2.33)	(2.38)	(0.49)	(2.51)	(0.56)
Target Weight		-0.064	0.625**	-0.067	1.498***	-0.006	-0.143	-0.183	0.234
		(-0.44)	(2.09)	(-0.28)	(2.80)	(-0.03)	(-0.56)	(-1.11)	(0.75)
Change in Leverage		0.002	-0.000	0.001	-0.002	0.002	0.001	0.003**	-0.003
		(1.29)	(-0.08)	(0.31)	(-0.42)	(1.17)	(0.52)	(2.12)	(-0.99)
Synergies Share		0.224	0.035	0.112	0.476	0.206	0.259	0.459***	-0.328
		(1.61)	(0.09)	(0.51)	(0.73)	(1.00)	(1.43)	(2.90)	(-1.11)
Index Inclusion		-0.033	0.025	-0.068		-0.056	0.003	-0.030	-0.013
		(-0.66)	(0.21)	(-0.83)		(-0.84)	(0.04)	(-0.51)	(-0.13)
Constant		0.040	-0.19*	0.045	-0.458***	0.035	0.044	0.049	0.019
		(0.77)	(-1.69)	(0.52)	(-2.79)	(0.55)	(0.48)	(0.83)	(0.18)
Observations	712	685	115	252	59	406	279	517	168
R-squared	0.009	0.016	0.129	0.005	0.193	0.025	0.010	0.042	0.020

Table 9

Regression results for the full sample

We present the regression results with *Excess Comovement* ($\beta_M - E(\beta)$) as our dependent variable. $\beta_A|\beta_T$ measures relative riskiness and is equal to (-1) for $\beta_A < \beta_T$ and 1 for $\beta_A > \beta_T$. $\beta_A|\beta_T \times Cash$, $\beta_A|\beta_T \times Mixed$ and $\beta_A|\beta_T \times Other$ are interaction variables of the dummy variables of cash, mixed and other deals with $\beta_A|\beta_T$. The base category in Regression (3) is pure stock deals. $\beta_A|\beta_T \times Target\ Inclusion$ is an interaction variable of Target Inclusion, a dummy equal to one in case the Target is included in the S&P500 Index as a result of the merger, with $\beta_A|\beta_T$. $\beta_A|\beta_T \times Different\ SIC$ is an interaction variable of Different SIC, a dummy equal to one in case the SIC of the acquirer and the target are different, with $\beta_A|\beta_T$. $\beta_A|\beta_T \times DifferentState$ is an interaction variable of Different State, a dummy equal to one in case the State of the acquirer and the the target are different, with $\beta_A|\beta_T$. *Target Weight* is the market capitalization of the the target company, adjusted for toeholds, relative to the combined market capitalization of the target and the acquirer firm eight weeks prior to the merger announcement. *Change in Leverage* is the difference in end-of-year leverage of the combined balance sheet (target and acquirer) one year prior to the merger announcement to the merged firm end-of-year leverage one year after the completion of the deal. Leverage is measured as book value of debt to total market value which is defined as book value of debt plus market value of equity. Change in Leverage is winsorized at the one and 99 percent level. *Synergies* is the ratio of the target's and acquirer's combined market adjusted abnormal value created over a six week window around the merger announcement (three weeks before to three weeks after) relative to the combined market value eight weeks prior to the merger announcement. *Index Inclusion* is a control variable equal to one if either the acquirer or the target changed their S&P 500 listing status from the beginning of the pre to the end of the post merger estimation window. All regressions control for a deal announcement year fixed-effect. T-statistics based on robust standard errors are calculated for a one sided test ($\beta_M - E(\beta) > 0$) and reported in brackets with significance levels of ten percent(*), five percent(**) and one percent(***).

Dependent Variable:	$\beta_M - E(\beta)$					
	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_A\|\beta_T$	0.052***	0.045**				
	(2.56)	(2.18)				
$\beta_A\|\beta_T$ x Cash			0.135***			
			(2.97)			
$\beta_A\|\beta_T$ x Other			0.055*			
			(1.43)			
$\beta_A\|\beta_T$ x Mixed			-0.002			
			(-0.04)			
$\beta_A\|\beta_T$ x Target Inclusion				0.132***		
				(2.38)		
$\beta_A\|\beta_T$ x Different SIC					0.066***	
					(2.41)	
$\beta_A\|\beta_T$ x Different State						0.057***
						(2.45)
Target Share		-0.064	-0.080	-0.079	-0.057	-0.072
		(-0.44)	(-0.54)	(-0.53)	(-0.39)	(-0.49)
Change in Leverage		0.002*	0.002	0.002*	0.002	0.002*
		(1.29)	(1.17)	(1.38)	(1.28)	(1.33)
Synergies Share		0.224*	0.212*	0.232**	0.229**	0.231**
		(1.61)	(1.51)	(1.66)	(1.65)	(1.66)
Index Inclusion		-0.033	-0.039	-0.040	-0.030	-0.032
		(-0.66)	(-0.78)	(-0.82)	(-0.61)	(-0.63)
Constant	0.04**	0.040	0.050	0.046*	0.037	0.040
	(1.95)	(0.77)	(0.96)	(0.87)	(0.71)	(0.78)
N	712	685	685	685	685	685
R-squared	0.009	0.016	0.023	0.014	0.017	0.017

Default Risk and Bondholder Wealth in US Mergers *

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Abstract

This paper provides a joint analysis of the effect of mergers on default risk and bondholder value. I find that mergers significantly increase acquirers default risk, measured by abnormal changes in CDS spreads. This change can be explained best by the deal structure, while target characteristics contribute only little. The impact of the merger on default risk is not only transitory, but shown to be persistent over time. It leads to a gain for CDS investors of USD 656m, while bondholders of acquiring firms lose USD 17.3m. This skewed distribution of gains and losses, as well as the positive relation between CDS notional outstanding and changes in default risk, provides an indication that empty creditors matter in terms of mergers.

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1 Introduction

Literature has comprehensively documented the value generation and destruction of mergers and acquisitions form the point of view of a firm's shareholders. The related work on how bondholders are impacted by mergers and acquisitions is much less developed and so far offers mostly ambiguous results, arguably because the trading data for bonds is not as complete as for equity and, moreover, because bonds are not as standardized an instrument as stocks. In this paper, I will take the analysis a step further by using a measure, namely credit default swap spreads, that is on the one hand independent of individual bond features and, on the other hand, has a high enough trading frequency to estimate the impact of the merger with a higher precision. Specifically, I intend to answer the questions of whether mergers are changing the default risk of corporate debt, how this potential change can be explained and how it effects bondholder value. Furthermore, this paper broadly adds to the growing literature on empty creditors by providing evidence on the relation between overinsurance and the change of bond default risk.

In the wake of a slowly recovering merger market, with many firms holding plenty of excess cash in their balance sheets, it is as important as ever to understand how mergers impact bondholders. Moreover, according to the Securities Industry and Financial Market Association (SIFMA), the volume of outstanding corporate debt in the US has been growing gradually and reached a volume of USD 8.47 trillion as of the second quarter 2012.[1] This compares to a market capitalization of all equities listed on the NYSE of USD 13.3 trillion as reported by the World Federation of Exchanges. A recent Bloomberg article notes that "forty-two percent of investment-grade companies that sold new issues in U.S. dollars last quarter cited mergers and acquisitions as a use of proceeds". The question of whether mergers change the risk of outstanding debt and hence lead to wealth-transfers between

[1] This is an increase by 5 percent compared to the second quarter of 2011 and by 40 percent compared to pre-crises level of 2007.

different stakeholders within a firm is therefore an important issue in the professional as well as the academic world. While the agency problem of debt (Jensen and Meckling, 1976), i.e. the cost of monitoring the compliance of the management's investment decisions with the ex-ante contracted strategy, is widely recognized, it is much less obvious why wealth transfers between share and debtholders actually happen. Are bondholders too lenient, maybe due to an overinsurance of their investment (empty creditors), in monitoring and restricting a firm's management from using mergers to redistribute wealth from bondholders to shareholders? Or will a merger lead to a safer entity by bringing together firms with less than perfectly correlated cash flows and thus shift wealth from shareholders to bondholders?

The contribution to the existing literature on bondholder wealth is threefold. First, by using a more liquid and more standardized measure, I extend past research in this field with an arguably more precise and powerful econometric analysis. Second, the close relation between CDS spreads and bond values allows to implicitly link changes in credit default swap spreads to changes in risk adjusted wealth of bondholders. This makes it possible to effectively put a price-tag on the impact of each merger transaction. Third, I provide preliminary evidence on how overinsured bondholders, i.e. empty creditors, impact M&A deals.

Combining the idea of the classical credit risk model by Merton (1974) with the no-arbitrage pricing framework for credit default swaps (see for, example, Duffie and Singleton (1999) or Hull and White (2000)), I propose several testable hypotheses on the impact of mergers on the default risk of bonds and hence bondholder value. The general prediction of this framework states that the change in default risk should be non-positive, depending on the correlation of the two merging firm's asset volatility. This is in line with the coinsurance or debt capacity hypothesis proposed by Lewellen (1971) and Higgins and Schall (1975). Indeed, I do find evidence in the data that firms merging across industry, where the correlation is arguably smaller, increase default risk significantly less than firms

merging within their industry.

Based on the analysis of 184 merger transactions with US acquirers, I find evidence that mergers increase CDS spreads on average. During a three day window around the merger announcement, the observed abnormal change in CDS spreads is on average +4.95 percent. This finding is statistically significant.

The overall increase in bondholder risk suggests a negative impact of M&A transactions on the acquiring firm's bondholder value. While the direction of the impact is consistent with Billett, King, and Mauer (2004), who found an abnormal drop in bond prices of -0.09 percent for investment grade bonds, and earlier work of Dennis and McConnell (1986), the magnitude observed in CDS spreads is substantially larger. In terms of wealth, this equals an average loss of approximately USD 17.3m for the acquiring firm's debtholders and a gain of USD 656m for investors holding CDS contracts. The bondholder value effect happens, because investors holding bonds in a firm that announces a merger will on average have to carry more risk on their investment going forward. Because this additional risk is not compensated by the market, they will effectively lose money.

To explain the significant average increase in default risk following a merger announcement, it must be the case that other important parameters change systematically. In my analysis, I differentiate between changes as a result of the *deal structure*, for example financing and consideration offered in the deal, and impacts that are due to *acquired risk and benefit*, as for example the leverage ratio or asset volatility of the target relative to the acquirer.

Overall, my findings suggest that mergers, on average, entail a considerable degree of wealth transfer between CDS investors and acquiring firm bondholders. Limited evidence also hints towards a wealth transfer from the acquiring firm's to the target firm's bondholders. I show that certain characteristics of the deal and the involved firms can be used to identify the transaction as desirable or harmful to existing debtholders of the acquiring firm. The remainder of the paper proceeds as follows. I describe the related literature

in Section 2. In Section 3, I develop the predictions concerning the effect of mergers on default risk and hence CDS prices. Section 4outlines the employed methodology and Section 5 describes the data used in the analysis. In Section I present the empirical results and Section concludes.

2 Literature

The literature has developed different reasons, from a shareholder point of view, for why mergers happen and what the predicted reaction of share prices following the transaction announcement is. The literature taking the bondholder's point of view, on the the other hand, has only received limited attention in answering the question of how mergers actually impact the value of the debt. The most prominent value proposition of mergers concerning debt has first been raised in the context of conglomerate mergers. The idea of coinsurance, the diversification of a firm's cash flow and hence the reduction of default risk, was introduced by theoretical work of Lewellen (1971) and Higgins and Schall (1975). They predict that coinsurance reduces risk, measured as cash flow volatility, in the merged entity and hence benefits existing bondholders. Early empirical literature on this topic goes back to Kim and McConnell (1977) who found a negative yet not significant abnormal bond return to merger announcements. While Eger (1983) found positive and significant abnormal returns, Dennis and McConnell (1986) found negative and significant changes. Positive and significant returns were again found by Maquieira, Megginson, and Nail (1998) while the most comprehensive analysis along in this strand of literature so far, by Billett, King, and Mauer (2004), only finds a significant positive abnormal bond return for target firms (suggesting a drop in risk as a result of the merger). For acquirers, they find a slightly negative price impact which would imply an increase in risk. An agreement of how bondholder wealth is impacted, however, has not been reached as the different findings were too diverse.

A major issue of all these studies is the sparse data on bond trades. The low trading frequency and limited sample size compromises statistical tests which were created for liquid stocks (c.f. Bessembinder, Kahle, Maxwell, and Xu (2009)). Moreover, unlike stocks, bonds cannot be treated as a homogenous instrument due to their many design features and different maturities. As such, the reactions to merger announcements vary considerably between issues and hence make conclusions on a general level dependent on strong assumptions.

Furfine and Rosen (2011) explore the question of how M&As effect bondholders not on the basis of bond prices, but from the point of view of a firm's probability of default. The latter is measured as a firm's expected default frequency, as calculated by KMV. They find that the acquirer's probability of default, on average, increases by 93.6 percent around a merger. This significant increase is assigned to a large extent to managerial actions and poor past firm performance. Because of the model-based default prediction, which relays on daily stock prices and quarterly leverage levels, the heterogeneity of bonds as well as the low bond trading frequency is not an issue in their analysis. While the research question and their general approach is similar to what is presented in this paper, I belief that the central element of both studies, the measure of default risk, differs considerably. The expected default score of Furfine and Rosen is based on a widely known model and predicts default within the next year. A CDS, on the other hand, is a market based measure of risk that is derived from actual transactions.[2] Moreover, the default horizon considered in my case is five years forward. Another notable difference between these studies is the ability to comment on bondholder value. The expected default score of Furfine and Rosen does not allow to effectively measure the price impact of a merger. CDS spreads, alternatively, permit to put a price tag on the merger impact and hence allow a classification of

[2] As such, CDS spreads should incorporate most of the current and forward looking information available for a firm and the entire market. This is a particularly important issue for large events with a big impact on a single firm, such as a merger announcement.

the effect's real magnitude. Additionally, expected default scores have the drawback that one cannot disentangle idiosyncratic changes in default risk from market-wide changes, an important property when determining the true impact of an event.

A comprehensive overview on credit default swaps is provided by Jarrow (2010). The existing empirical literature using CDS data as a main source of information is comparably sparse. Much of the early work using CDS data focussed on the limited-arbitrage between bond and CDS prices. Hull, Predescu, and White (2004), for example, look at the reactions of these two instruments to credit rating changes. Longstaff, Mithal, and Neis (2005) conclude in their research that default risk is the major component of a corporate yield spread and as such important for driving price movements. Blanco, Brennan, and Marsh (2005) as well as Feldhütter and Lando (2008) show that the no-arbitrage parity relation between credit spreads and CDS hold on average, with deviations occurring in short time windows. During these deviations, CDS prices lead bond spreads (Zhu, 2006). Empirical work with a more corporate oriented focus has emerged with a broader CDS coverage of firms. Acharya and Johnson (2007), for example, used CDS to analyze insider trading while Jorion and Zhang (2007, 2009) use CDS to measure credit contagion, i.e. the clustering of default of related firms. Stulz (2010) considers the role of CDS in the recent credit crises and concludes that the CDS market is not responsible for the extraordinary events which defined the crisis, but, especially in the beginning of the crises, seemed to have worked rather well. Another important strand of literature using CDS is on empty creditors, i.e. bondholders that have bought full insurance against default. Bolton and Oehmke (2011) provide a theoretical model of how CDS may influence the behavior of bondholders and Danis (2012) provides first empirical evidence on how empty creditors matter in case a firm goes into default.

3 Hypotheses

The hypotheses in this study are centered around changes in credit default swap spreads and hence it is important to understand the basic nature of this instrument first. In the classical view, a CDS protects its buyer against the loss of funds invested in a specific reference entity. In case of a *credit event* of the reference entity, for example a missed coupon payment or a straight out bankruptcy, a CDS is triggered and the protection buyer receives the difference of par minus recovery, multiplied by the insured notional amount.[3] For this protection, the buyer has to pay an annual fee, measured in basis points of the insured nominal, to the seller.[4] The minimum nominal value that protection can be bought for is USD 10 million. While many of the largest firms worldwide use CDS to manage their risk, the protection seller side is dominated by large, global financial institutions that are, according to the Bank for International Settlements, liable for more almost 75 percent of the outstanding net notional as of June 2012.

The CDS market has experienced an impressive growth in the last decade. According to Markit, the gross notional amount outstanding has been growing from USD 919 billion in 2001 to USD 25.9 trillion in 2011. After a netting of counterparty contracts, the outstanding amount as of 2011 was still USD 2.7 trillion. The large size and increasing complexity of this market had led participants to agree on a general institutional framework, which has become known as the Big Bang Protocol (ISDA, 2009).[5] It largely standardizes the way CDS contracts are structured and provides settlement guidance in case of default.

[3]There exist four different standards on what characterizes a credit event (old restructuring, modified restructuring, modified-modified restructuring, and no restructuring). The difference between the first three definitions mainly concentrates on the classification of the debt that is deliverable upon a credit event. The fourth, no restructuring, excludes a restructuring of debt, as for example a Chapter 11 filing would be, from the credit event definition. In the US, no restructuring has become the most widely used standard.

[4]For US entities, the introduction of the Big Bang Protocol (2009) has led to the general agreement to split the spread into a fixed coupon payment of 100bps (investment grade) and 500bps (high yield) and pay the remaining spread as an upfront payment to the seller.

[5]For a a detailed summary of the main content of this protocol, see Price and Casey (2009).

Credit default swaps have also been trading in indices since 2001. The two most liquid families of indeces are the CDX for North America and the iTraxx for Europe and Asia. In case of an index, the notional is split evenly across the constituents. In case a reference entity of an index with n constituents defaults, the protection buyer will receive $\frac{1}{n}$th of the nominal invested, less any recovery proceedings.

The spreads quoted in the single name CDS market are the costs incurred to protect all debt issued at the level of a reference entity (and not for a specific bond issue only). For larger entities, the protection is sometimes split into a senior and a subordinated debt CDS. This aggregation at the level of a firm's debt class is crucial for my analysis, as it allows the assessment of the *overall* change in default risk of a firm.

To validate that credit default swap spreads are indeed an appropriate way to think about changes in debt default risk and value, it is illustrative to consider the asset-swap CDS pricing model. Figure 1 highlights the basic transactions involved in this no-arbitrage pricing framework. Initially, the investor seeking default protection takes up a loan at Libor (r_l) and uses the proceeds to buy a risky τ-year corporate bond B_i at par, with a nominal of N_i and a coupon of r_i. To hedge against interest rate changes, the buyer engages in a fixed-for-float interest rate swap at cost c_{SWAP}, paying r_i and receiving r_{float}.

FIGURE 1 ABOUT HERE

The difference in coupon payments between what the investor receives from the risky bond after hedging for interest rate changes and what he has to pay for the risk-free loan, $r_{float} - r_l$, will then equal the price to protect B_i against default (r_{cds}). The asset swap model, therefore, requires that the following no-arbitrage condition holds:

$$r_{float} - r_l \; = \; r_{cds} \tag{1}$$

Assuming the cost for the interest rate swap, $c_{SWAP} = r_i - r_{float}$, and the Libor are deter-

mined exogenously to the firm and remain stable over the short time horizon, the price for an individual firm's credit default swap is, as seen in equation 1, essentially driven by idiosyncratic changes in risk, reflected in r_i:[6]

$$r_i - c_{SWAP} - r_l = r_{cds} \tag{2}$$

To develop predictions on the impact of mergers on bondholder wealth, I combine the before mentioned replication argument with the basic idea of there credit risk model by Merton (1974). At this point, it is important to notice that the goal of this exercise is not to find anomalies in CDS pricing or even propose a new pricing model, but rather to provide a foundation to think in a structured way about the different channels through which a merger may impact CDS prices; the structural model offers just that, a simple, yet powerful tool for comparative statics. I start with the notion that, based on their payout profiles, a risky bond of firm i can be replicated by buying a risk-free bond and selling a put-option on firm i's assets:

$$\text{Payoff Risky Debt} \equiv \text{Payoff Riskfree Debt} - \text{Payoff Put Option}$$

With the risk-free interest rate being determined exogenously and assumed constant over short periods of time, the firm specific component of the bond value is determined by the value of the put option alone and hence can be priced according to the formula of

[6]Changes in risk should therefore also be directly observable in bond prices. However, the low trading intensity in most of the bond market suggests that these changes in risk are generally only reflected gradually in bond prices (Bessembinder, Kahle, Maxwell, and Xu (2009)). Observing the more actively traded derivatives market should therefore be preferred if the market reaction to a specific event is considered.

Black and Scholes (1973):

$$B_i = A_i \cdot N(-d_1) + K_i \cdot e^{-r_f(\tau)} \cdot N(d_2),$$

with

$$d_1 = \frac{log(\frac{V_i}{K_i}) + r_f(\tau) + \frac{1}{2}\sigma_A^2(\tau)}{\sigma\sqrt{\tau}}, \quad d_2 = d_1 - \sigma_A\sqrt{\tau}.$$

where A_i are a firm's total assets, σ_A the firm's asset volatility, K the nominal value of a firm's total debt, τ the time to maturity and r_f a risk-free interest rate. $N(\cdot)$ is the cumulative standard normal distribution function.

As the market value of the risky corporate bond (B_i), determined by the put option value, is closely connected to the coupon payment (r_i), relevant in the asset swap model, the two arguments can be linked.[7] This allows a comparative static argument along the following lines: as the value of the long put-option increases, the market value of the bond, B_i, decreases or, put differently, the coupon of this bond would have to adjust upwards for the market value to stay equal[8]. A higher coupon on the corporate bond will increase the CDS spread ceteris paribus.

My hypotheses can hence be arranged along the input parameters of the Black Scholes option pricing model. In the context of M&A, the transaction between acquirer (a) and target (t) can be interpreted, under the assumption that both firms assets follow the same brownian motion, as a combination of two put options. To single out the effect of the merger, the parameter comparison should be done between the ex-ante proforma combi-

[7]Because the Merton model assumes a zero-coupon discount bond, the implicit coupon return can be derived from the difference between the nominal value K and the market value of the bond B. In other words, the larger the difference between B and K, the higher the implicit coupon r_i.

[8]Another way to think about it is by assuming that a firm issues a bond with exactly the same duration and notional everyday anew. If the default risk of the firm increases, the firm would have to issue the next bond with a higher interest rate for a bond issued at par.

nation P_m^{pre} and the ex-post merged firm P_m^{post}:

$$\begin{aligned} & P_m^{pre}(A_a + A_t, K_a + K_t, r_f, (w_a^2\sigma_{A,a}^2 + w_t^2\sigma_{A,t}^2 + 2w_a w_t \rho_{a,t}\sigma_{A,a}\sigma_{A,t})^{\frac{1}{2}}, \tau) \\ & \Leftrightarrow \\ & P_m^{post}(A_m, K_m, r_f, \sigma_{A,m}, \tau), \end{aligned} \tag{3}$$

where $\rho_{a,t}$ measures the correlation between the changes in assets of the two firms. The relation in equation 3 thus provides a basic framework for predictions on merger related impacts on CDS spreads. While it is not immediately clear why a merger should lead to a change in the default probability of a firm, equation 3 provides some guidance on what channels are important to consider in this setting.

The notion of co-insurance, a cash flow diversification benefit obtained through a merger, also holds for the CDS framework. It predicts that the merger of two weakly correlated firms reduces the default risk of the new entity. A low $\rho_{a,t}$ reduces the value of the put and increases the value of the bond or, alternatively, lowers the coupon. A lower r_i reduces the spread of the CDS and hence the default risk as perceived by the market. Hence, even if the merger creates no synergies and does not change the level of total debt, a change in default risk is possible. This leads to the before mentioned general prediction that the effect of mergers on default risk should a priori be non-positive.

A reason for shareholders to engage in a merger transaction are expected synergies. These newly created assets belong to the residual claimants of the firm and hence not only increase the value for shareholders, but at the same time also benefit bondholders. In the analyzed framework, positive merger related synergies (A_{syn}) imply that the asset base of the merged firm is larger than the ex-ante proforma combination of acquirer and target, i.e. $A_a + A_t + A_{syn} = A_m > A_a + A_t$, and hence decreasing the price of the put option. The resulting increase in the value of the bond reduces the required coupon payments and hence lowers the CDS spread.

Ghosh and Jain (2000) conclude in their analysis that mergers on average increase the leverage of the merged firm relative to the ex-ante proforma combination of the two entities involved in the transaction. An increase in leverage as part of the merger is commonly perceived as an increase in the firm's default risk. This is also reflected in equation 3, in terms of book-leverage. If the acquirer raises additional debt to finance a transaction (K_{new}), the merged firm ends up with $K_a + K_t + K_{new} = K_m > K_a + K_t$ and hence an increase in the price of the put option. This leads to a lower bond value and thus a higher CDS spread.

Another hypothesis concerns the consideration offered in the transaction. Pure cash mergers, i.e. mergers are paid with a 100 percent cash consideration, will lead to a drain in the acquirer's cash holdings and hence a reduction in its assets, $A_a + A_t > A_m$. Lower assets will increase the price of the put option and reduce the value of the bond. As a result, I should observe an increase in CDS spreads for cash mergers.[9] In contrast, mergers that only offer stock consideration should leave total assets unchanged. Pure stock mergers should therefore not lead so significant changes in a firm's default risk.

An important issue in the literature on bonds are covenants (see, for example, Dichev and Skinner (2002) or Chava, Kumar, and Warga (2010)). Covenants are generally amended to protect the owner of bonds from excessive risk taking by shareholders/management and thus reduce agency conflicts. As covenants exist in many different forms and requirements, I focus my analysis on some of the most influential ones in relation to a merger. For the acquiring firm, I consider the put covenant which allows current debt investors to call their bonds at par-value if they perceive that the firm's behavior is not consistent with the ex-ante agreement. For the target firm, the covenant I consider is change-in-control. This covenant allows, in a similar fashion, to call the bond at par value in case the firm's

[9] A second order effect, reinforcing the impact of the reduced assets, comes from the asset volatility. Cash positions contribute a very small amount to asset volatility and a reduction of this position should hence lead to a higher asset volatility of the firm.

ownership changes.

Both covenants reduce the risk carried by bondholders as they protect them from a transfer of wealth away from them by factors other than the regular business risk. From an incentive point of view, the interests of acquiring and target firm bondholders associated with these covenants are aligned in the sense that neither of them favors a deal that impairs the bondholders of the merged firm. As these covenants provide a very broad protection, the impact cannot be attributed to a single parameter in equation 3 alone. Therefore, I predict that firms with a higher share of their bonds covered by such a covenant, on average, experience a lower impairment of their wealth from merger transactions than firms with no coverage and should therefore exhibit a non-positive effect on their CDS spreads.

4 Methodology

Hypotheses are tested using an event-study methodology setting as applied commonly with stock return data. Compared to the monthly data used in most of the previous literature on bondholder wealth, the daily spreads of CDS allow for a much more precise measurement of the market reaction. According to the analysis by Bessembinder, Kahle, Maxwell, and Xu (2009), the market model approach is able to provide higher powered tests as for example the market adjusted method used in Jorion and Zhang (2009). The drawback, however, is that the market model requires a much more complete time series of data for calibration. I believe that the market model is the more appropriate one for the events I consider here, as it allows to control for the firm-specific volatility in CDS spreads and thus does not overweight firms with an a priori higher spread volatility. I calculate the abnormal change of a firm's CDS spread around a merger announcement (Abnormal Spread Change (ASC) or Cumulative Abnormal Spread Change (CASC) for a multi-day window) using the market model. The abnormal change of firm i's CDS spread is hence

the residual of the following regression:

$$\Delta r_{i,t} = \alpha_i + \beta_i(\Delta r_{m,t}) + \epsilon_i, \tag{4}$$

where $\Delta r_{i,t}$ is the relative change in the CDS spread of firm i and $\Delta r_{m,t}$ is the relative change in the spread of the CDS market index (CDX North America IG). I define the estimation window for the sensitivity parameter β_i to be 100 days, ending at the merger run-up window 20 days before the announcement [-120,-20]. The length of the event window, the period where the market is expected to reassess the risk of the reference entity, is chosen to be from the day prior to the day after the announcement [-1,+1]. I take the logarithm of the CASC's because a closer inspection of the data revealed that log CASC's follow more closely a normal distribution and as such are more desirable for statistical testing.

The hypotheses stated in Section 3 are tested in two ways; univariate, by comparing sample splits along different variables of interest and multivariate, by regressing the variables of interest on CASCs. In the former case, it is especially important to consider the derivation of an appropriate standard deviation to test for significance. Bernard (1987) raised the issue that firms within the same portfolio split may breach the independence assumption of ASC's between firms and thus bias the standard deviation downwards. This could be the case even if the firms in the portfolio do not share the same actual event day. One way to adjust for this issue is, according to Bernard and Kothari and Warner (2007), to equal weight the firms within their portfolio and hence provide a structure that explicitly allows for the dependence between the firm's ASC's. For testing purposes, one should then use the standard deviation of the portfolio.

Due to limited availability of CDS data for target firms (cf. Section 5), the main analysis is based on CDS data from acquiring firms only. It is therefore important, especially for the multivariate analysis, to not only control for deal implied ex-ante to ex-post changes,

but also consider the ex-ante differences between acquirer and target firms.

While it is important to determine the magnitude and statistical significance of the merger impact in the first place, it is of equal substance to put a price tag on the effect and relate the statistical finding to real values. Changes measured in USD are important as they allow a common denominator for direct (CDS holders) and indirect (bondholders) impacts and hence a figure that sums up the total change in value implied by the merger. Changing CDS spreads directly impact the wealth of investors already holding CDS; these investors will either pay too much, if default risk decreases, or too little, if default risk increases, going forward. The same argument holds for the indirect impact on bondholders. If default risk increases, current bondholders will receive too low an interest rate for the risk they are taking going forward, while a decrease in default risk will lead to a relative gain.[10] A possible conflict of interest that can evolve from these payoff-profiles are discussed in Section 6.9 An approximation of the hypothetical change in bondholder wealth[11] can be derived by taking the difference of CDS spreads before (pre) and after ($post$) the transaction:

$$
\begin{aligned}
r^{CDS}_{pre} &= r^{i}_{pre} - c^{SWAP}_{pre} - r^{l}_{pre} \\
r^{CDS}_{post} &= r^{i}_{post} - c^{SWAP}_{post} - r^{l}_{post} \\
&\Leftrightarrow \\
r^{CDS}_{post} - r^{CDS}_{pre} &= (r^{i}_{post} - r^{i}_{pre}) - (c^{SWAP}_{post} + r^{l}_{post} - c^{SWAP}_{pre} - r^{l}_{pre}) \qquad (5)
\end{aligned}
$$

Provided the assumption of a constant risk-free rate and a constant cost for swap's holds

[10] For this line of argument to hold, I have to assume that bonds are priced correctly prior to the merger, with unexpected changes in risk as large as the ones implied by a merger not priced in. For cases where the CASC is significant, it seems unlikely that such a change in risk has already been priced in at issuance.

[11] Provided this jump in default risk is persistent (c.f. Section 6.8), the gain/loss calculation for a current debt investor holding the asset until maturity would have to reflect this impact over the remaining debt maturity, as future coupon payments of current issues will not be adequate for the asset's default risk.

over the short three day window, the change in CDS spreads is approximately comparable to the changes that should be reflected in debt of the same maturity.[12] Because equation 5 only holds for absolute changes in CDS spreads, CASC's have to be transferred back into basis points through multiplication with the CDS spread level prior to the announcement. Additionally, if the change in default risk can be shown to be persistent (c.f. Section 6.8), a calculation of the total change in value has to reflect the duration of the debt currently in the portfolio. Put differently, the bondholder will not only receive too low a coupon for the default risk taken in the current year, but for all future years invested until repayment.

5 Data

Data on Credit Default Swap spreads is obtained from Bloomberg. I use the end-of-day New York spread data which is available for more than 800 US firms. As CDS are only traded over-the-counter, prices are not as readily available as for stocks. Similar to other data providers, such as Markit or CMA, Bloomberg collects daily tradable CDS spread quotes during a window of 24 hours from the most active CDS contributors, removes the highest and lowest if at least five quotes were contributed and reports the arithmetic mean. In case no provider offers a tradable quote, no price is reported for that day. Bloomberg provides data on CDS starting 2004, with a growing universe of entities being covered in more recent times.

Because CDS coverage predominantly centers around the largest firms in the S&P 500, the availability of target firm CDS is considerably limited (25 firms only). The main analysis is therefore conducted with acquiring firm's CDS only and limited to the more widely available senior tranche. CDS can be bought for a variety of durations, from a single month to many years. While the recent trend has been towards shorter maturities, the most ac-

[12] Debt is rarely of a maturity that is exactly five years. For debt with a maturity longer than five years, the change in CDS spreads is likely to be a lower bound while for debt with a shorter than five year maturity, CDS spread changes offer an upper bound.

tively traded tranche is still the five year duration which I use in my analysis.

My data from Bloomberg lists 929 firms with a CDS spread. After reducing the sample by CDS on the subordinated bond class and keeping only CDS with the no restructuring credit event definition, my data covers 830 entities.

The index relevant for this study is the CDX North America Investment Grade. The index is composed of the 125 most liquid single name CDS and rolled every six month according to the most liquid CDS titles in the market. While old index compositions keep being traded, the liquidity is concentrated in the most current composition. As with the single name CDS data, I also obtain quotes of the index from Bloomberg. For the main analysis, I rely on the most current and hence most liquid index composition.

The price data from Bloomberg is extended by volume and number of transactions data obtained from the Depository Trust & Clearing Corporation (DTCC). This data is provided on a weekly basis, starting in November 2008. DTCC only reports levels of number of contracts and nominal value outstanding (and not the effectively observed changes in contracts or nominal value). Therefore, these numbers need to be interpreted as a lower limit of total transactions as they represent only the residuals after netting out total market activity.

TABLE 1 ABOUT HERE

Table 1 provides an overview of the trading activity and changes in nominal volume outstanding of CDS used in this analysis. The average number of trades conducted in the week of the event is 37.88[13], around 8 trades a day, with an average weekly change in gross volume outstanding of USD 38.8m. The outstanding gross notional amount of credit default swaps for acquiring firms one week prior to announcement is USD 13.3bn.

[13]The change in number of trades is a lower bound for the deals closed and hence the minimum value of 0 in the statistic should not be taken as a sign that no deal has taken place.

5.1 Further Data

Data on deal characteristics of mergers and acquisitions are from the SDC Platinum database. I restrict deals to public acquirers that are located within the US and are announced in the period of 2004 to 2011.

Across Industry transactions are classified as deals in which the acquirer and the target company operate in different industries. Classification of industry affiliation is according to SIC with data provided by Compustat.

Synergies are defined similar to Brealey, Cooper, and Kaplanis (2010), as the cumulated abnormal stock return in the eleven day event window [-5,+5] around the merger announcement, multiplied with the market value six days prior to the merger announcement. Abnormal stock returns are calculated using the market model based on the value-weighted CRSP index. Stock market returns and equity values used in this calculation are obtained from CRSP.

Compustat also provides data on debt outstanding to calculate *Leverage*. Following Ghosh and Jain (2000), debt is defined as the book value of long-term debt (Compustat item 'dltt') and debt included in current liabilities (Compustat item 'dlc') and set in relation to total firm assets, calculated as book value of debt plus market value of equity.[14] For target firms outside the US that or not reported in Compustat Global, data on debt and cash is from Reuters Datastream.

I further use Bloomberg to obtain data on each firm's debt *Covenants* as well as the *Time to Maturity* of a firm's outstanding debt. For covenants, I form the ratio of the amount of debt capital protected to total debt capital outstanding for each firm while for time to maturity, I calculate a value-weighted average over all bonds of a firm.

Data on *Asset Volatility* are derived by own calculations. I apply the iterative approach

[14] A second measure of leverage, as suggested amongst others by Welch (2011), is calculated using net debt, i.e. book debt adjusted by cash holdings (Compustat item 'ch'). The results are similar, both in terms of magnitude as well as statistical significance and hence I refrain from reporting them separately.

used in Vassalou and Xing (2004) to calculate a firm's daily asset value over time and derive its asset volatility. The approach is outlined in more detail in Appendix A.

To derive an estimate of the firm's *Asset Beta*, I value weight the equity beta, calculated over 250 trading days ending 20 days prior to announcement, and the debt beta, which I assume to be 0.4 following Kaplan and Stein (1990), by the end-of-year leverage level prior to the announcement year.

Data on *Consideration* is obtained from SDC using the items 'percent of cash' and 'percent of stock'. Also from SDC is the variable *Share Acquired*, which is derived from the item 'percent sought'.

Deal Ratio is calculated as deal value reported by the SDC item 'value of transaction' (paid consideration plus debt outstanding of target firm) divided by the acquiring firm's market value of equity 20 days prior to the announcement. For 33 deals that did not report a deal value, I proxy value of transaction by multiplying the variable 'Share Acquired' with the market value of the target at the day of announcement and added net debt.[15]

The initial sample from SDC, in line with the before mentioned restrictions, covers 8'878 deals. I require that at least one of the involved firms, acquirer or target, has CDS price data available around the merger announcement. Specifically, I drop all transactions that have less than 60 CDS spread observations in the 100 day pre-runup window [-120,-20] and less than two observations during the three-day announcement window [-1,1]. This leaves 449 deals. To be recognized as an important enough transaction by investors, I limit the variable 'Deal Ratio' to be at least 1 percent and require that 'Share Acquired' is least 30 percent. This reduces the sample to 184 transactions, 25 of which have CDS spread data for target firms. The sample reduces further due to data limitations in parts of the analysis. For the analysis of CDS investor value, the sample reduces to 59 observations

[15] An alternative measure for deal ratio would be to consider only the equity value purchased in the transaction ('Share Acquired' multiplied with the market value of the target at the day of announcement) relative to the acquiring firm's market value of equity. The results are similar in terms of magnitude and significance and can be provided upon request.

due to limitations in the DTCC coverage.

Descriptive statistics for the main variables can be found in Table 2. Several statistics are worth noting. The average level of CDS spreads in the week prior to the deal announcement is considerably lower for acquirers (113.28 bps) than for targets (163.34 bps). However, due to the before mentioned constraint of CDS data availability, the number of observations for the latter group is rather low. Consistent with the relative size of the CDS spreads, the asset volatility of the acquirer, 16.35 percent, is notably lower than that of the target with 21.85 percent. On average, the acquired firm is 27.07 percent of the acquiring firm's value of equity. Because the ratio is calculated based on the target's enterprise value, it can be quite large. The average stake that the acquirer purchases in the target firm is 96.63 percent.

TABLE 2 ABOUT HERE

Pairwise correlations of the main variables of interest are provided in Table 3. While most explanatory variables seem to be only weakly correlated, there are some notable exceptions that need to be considered with care. It seems, for example, that acquirers with a relatively higher ex-ante leverage ratio in general pick targets with comparably higher leverage ratios too (correlation of 0.514). An similar observation is made for asset volatility, where the correlation between acquirer and target is 0.498. Because neither of the two pairs of correlated variables are used together in the same regression, the concerns are only minor. However, the rather strong negative correlation of -0.547 observed between consideration offered and deal ratio is used explicitly in some of the regressions. While this correlation is a common observation in practice, as larger deals generally are financed with a bigger share of stock, the size of it has to be kept in mind when considering the regression

6 Empirical Results

6.1 Announcement Reaction

I start by documenting the overall reaction of CDS spreads to merger announcements. Figure 2 shows the development of the acquirer and target firm's average CASC around the deal announcement. Both participants in the transaction show a distinctive reaction to the event.

FIGURE 1 ABOUT HERE

The CASC of an equal-weighted portfolio of acquiring firms in the three day window bracketing the deal's announcement is +4.95 percent and statistically significant. For the target portfolio, the reaction is much more pronounced, with a significant CASC of -19.31 percent. The average, unadjusted CDS spread raises by 9.57 basis points (+9.2 percent) in case of the acquirers and drops by 19.76 basis points (-11.62 percent) in case of the targets. In terms of value, the increase in default risk impairs existing bondholders of the acquiring company, on average, by USD 17.3m, while the drop in default risk benefits target bondholders, on average, by 86.3m.

I next explore the hypotheses of Section 3 in two ways: first, by comparing sample splits along a variable of interest (c.f. Table 4) and second, by using a multivariate regression framework (c.f. Tables 5 and 6).

Before considering the hypotheses, a general remark should be made on the control variables. The relative size of the deal is an important determinant of the effect. As shown in panel A.1 of Table 4, deals with an above median Deal Ratio have a CASC of 8.32 percent while small deals only have a CASC of 0.69 percent. This effect is confirmed in the basic regression framework of Table 5 where an increase in the Deal Ratio by one percentage point increases CASC by 4.4 basis points. To control for the notion that more risky firms

may initiate more risky deals, I include a variable measuring acquirer asset volatility. The coefficient shows up positively in the regression, yet has no significant effect on CASC. The control for the big bang agreement was because it was hard to grasp how the institutional changes in the market effect the trading and pricing behavior. The agreement had no significant impact on CASCs and hence no additional measures need to be taken.

6.2 Coinsurance

So far, the empirical research has found it difficult to provide significant evidence in support of the coinsurance theory related to bond returns. Even though Billett, King, and Mauer (2004) found an effect that points in the right direction, i.e. a larger increase in abnormal bond returns for within-industry mergers, they were not able to show that this coefficient is statistically different from zero. Furfine and Rosen (2011), on the other hand, find an increase in expected default risk for firms merging across industries, a result that is pivotal to the general expectation. Splitting the sample along the four-digit SIC code, I find that mergers within the same industry increase risk significantly, CASC of 8.91 percent, whereas across industry mergers react considerably less, with a CASC of 3.49 percent (c.f. panel A.2 in Table 4). The difference is significant at the one percent level and a first indication for the coinsurance effect. That there is a difference in reaction to the merger announcement can also be shown graphically. In panel (a) of Figure 3, the difference in announcement reaction of within and across industry deals shows up markedly. The regression confirms this finding in column 1 of Table 5 and shows it to be robust against the inclusion of the most important controls (c.f. Table 6). Deals that are done across industries will on average increase CDS spreads by 4.2 percentage points less than deals that are done within the same industry. In terms of value, debt investors of firm's that merge within the same industry, on average, face costs of USD 41.9m. This is a 4.5 fold higher cost than what bondholders of firms which merge across industries have to bear (USD 9.4m).

Table 4 ABOUT HERE

6.3 Synergies

To conform with the hypothesis on synergies of Section 3, I focus on the sum of target and acquirer synergies (total synergies), measured in USD. While target synergies are a direct measure of the expectations of the acquiring firm's management[16], acquirer synergies generally reflect the market's evaluation of how large the synergy asset will be eventually.[17]

For the univariate analysis, I split the sample into transactions with positive and negative synergies. Contrary to my predictions, positive synergies lead to a significant abnormal increase in CDS spreads by 6.29 percent while negative synergies have a much lower reaction of 3.14 percent only (c.f. panel A.3 in Table 4). The regression results, where I control for the total amount of synergies created, statistically confirm the univariate findings. However, in economic terms, the effect is almost negligible; for every billion of USD in synergies created, CASC increases by 0.8 percentage points (c.f. column 3 of Table 5). Nonetheless, the fact that the finding is not in line with the prediction requires an explanation.

Provided that the proxy for synergies I use is adequate, I believe that a possible answer to this result can be found in the investor's perception on synergies' riskiness. If they believe that management's assessment of synergies will not materialize, the money spent on synergies, i.e. the premium paid to target shareholders, will not increase total assets of the firm in the long run, but rather reduce them and hence increase default risk. A

[16]The size of the premium the acquiring firm's management is willing to pay for the acquisition can be interpreted as the lower bound of what they expect to gain from the merger.

[17]Other argumentation may be in favor of considering the acquiring firm's synergies only as those are the ones which will stay within the merged firm. Target synergies are, for a large part, due to premiums paid to target shareholders and will thus only show up partially in the merged firm's assets. However, I believe that after controlling for consideration offered, total synergies are a better measure as they combine the informed guess of management with a critical market assessment. Consideration is important, because it is only in all-cash mergers that synergies vanish completely and only in all-stock mergers that they are retained fully within the merged firm.

possibility to test this is to split total synergies into target and acquirer synergies, as it is predominantly the former that is perceived to be more risky (in the sense that I argued above). In untabulated regressions, I find confirming evidence to this hypothesis, as only target synergies have a statistically significant and positive impact on CASCs. Moreover, controlling for consideration offered (cash-only vs. stock-only) and using a cross-term of consideration and synergies, I find that the increase in CASCs is largely driven by target synergies in deals that are paid for in cash only. In other words, those deals in which the paid synergies will not increase the assets of the merged firm.

Figure 3 ABOUT HERE

6.4 Leverage

The data confirms the hypotheses that deals which increase the leverage of the acquirer also lead to positive CASCs. A first indication thereof can be seen in panel (b) of Figure 3. The increase in CASC is considerably steeper for acquirers that are subject to an increase in leverage than for those which see a decreasing leverage level. An increase in leverage, on average, results in a CASC of 6.14 percent. This is significantly different from the leverage decreasing deals which have no significant impact on default risk (c.f. panel A.4. in Table 4). Not surprisingly, this result is confirmed in panel A.5. of Table 4 where the sample is split along changes in book debt (increase/decrease). This finding also holds after controlling for other deal specifications. As shown in column 2 of Table 6 an increase in leverage by one percentage point significantly increases CASC by 0.3 percentage points.[18] Thus, on average, leverage would have to increase by 16.3 percentage points to compensate, ceteris paribus, for the reduction in default risk through coinsurance. In terms of value, acquiring firms that increase their leverage by one percentage point, on average, reduce their bondholder wealth by USD 0.7m.

[18]Similar results in terms of magnitude and significance are obtained if leverage is considered net of a firm's cash holdings.

Figure 5 ABOUT HERE

6.5 Consideration

Consistent with the prediction, I find that mergers paid with 100 percent cash significantly increase default risk relative to mergers paid with stock-only. CASC for all-cash deals is 3.93 percent and significant, while deals that are paid with stock-only reduce default risk by 2.24 percent, yet are not statistically significant (c.f. panel A.6 in Table 4). However, due to data availability, this result is subject to a limited sample size and hence implications thereof have to be considered with care. Even though this result is confirmed in the regression analysis in column 5 of Table 5, with cash deals increasing CASC on average by 9.6 percentage points more than stock deals, the total impact of consideration offered diminishes and turn insignificant after the main variables of interest are controlled for (column 3 in Table 6) .

Figure 6 ABOUT HERE

6.6 Covenants

Both covenants that are considered in this analysis are fairly expensive for firms if they are triggered and thus in general undesirable to breach. Consistent with the prediction of Section 3, I find that firms with more than half of their notional value protected by covenants increase CASC significantly less than firms with a low protection level. However, the two covenants differ in how they effect the firm's risk as can be seen in the sample splits of Table 4. While firms with a high put coverage ratio seems to decrease their default risk, even though not significantly, the firms with low ratio have a highly significant CASC of 6.22 percent. Similarly, the higher availability of CIC covenants in target firms result in a lower CASC for the acquiring firms, however the difference to the firms with a low coverage is, even though statistically significant, economically considerably lower than in their

put covenant counterpart. The difference of impact between the two covenants becomes more obvious in the regression analysis. Acquiring firm put covenants are decreasing CASC significantly by 4.2 percent and are robust to the inclusion of the most important control variables column 7 in Table 5 and column 5 in Table 6, while the impact of CIC covenants is not significant in either regression setting (c.f. column 6 in Table 5 and column 4 in Table 6).

6.7 Ex-ante differences

An acquirer's default risk may not only change as a result of the structure of the deal, but also adjust to the risk purchased in the transaction. In this case, the target has a higher default probability than the acquirer prior to the deal. Because CDS spreads for target firms are only available for a very limited sample, I use four proxies to test this channel of risk transfer.

Table 7 ABOUT HERE

The univariate results, collected in panels B.1 to B.4 of Table 4, positively support the notion of acquired risk in three out of the four cases. For deals in which targets have a higher asset volatility, a higher leverage or a lower bond duration[19] relative to the acquirer, the CASC is significantly higher. The graphical analysis of panels (a) and (b) in Figure 4 underlines this finding. In both cases, CASCs increased considerably more if the target firm was perceived more risky than the acquirer. The direction of impact of these results carries over to the regression analysis in columns 1 to 4 in Table 7. However, after controlling for even the very basic deal characteristics, none of these proxies is able to explain a significant share of the total change in CASC. The results in columns 5 to 8 in Table 7 put

[19]Target bonds with a lower duration can be thought of as gaining in seniority relative to existing acquirer bonds in the sense of a repayment priority given the acquirer's asset base.

the two explanatory approaches of deal structure and bough-in risk in relation. The former variables are clearly of higher explanatory power and keep, for most of the analysis, their significance while the latter remain insignificant. Hence, changes in acquiring firm's CDS spreads are better explained by deal characteristics than by relative differences.

Figure 4 ABOUT HERE

6.8 Longterm Effects

A concern of the previous analysis may be that the reaction in CDS spreads observed around mergers is only a short-lived deviation from the true prices and hence the relative low trading volume in bonds. If this were the case, calculations on wealth changes and shifts would be temporary only and disappear as quickly as they have shown up. A first indication that speaks against the effect being temporary only is the graphical representation of CASCs around the event. Even though limited in the number of days it covers, Figure 2 displays a fairly steady development of CASC before and, more importantly, after the announcement. To address this issue more rigorously, I follow previous literature on long-term return analysis such as Fama (1998) and Mitchell and Stafford (2000). These authors strongly advocate the use of calendar-time portfolios to analyze performance in the long-run. Specifically, they show that calendar-time portfolios are more powerful to detect truly significant abnormal returns over longer event-windows than the more traditional buy-and-hold approach.[20] For each day within the window of interest (2004 - 2011), I construct portfolios by equal weighting the firms that are within the event window that I define to last from three days prior to the merger announcement to 40 days after merger completion. The changes in portfolio CDS spreads ($\Delta r_{m,t}$) are then regressed on

[20]The buy-and-hold approach does generally not allow to control for the cross-sectional dependence of the firms involved in the event of interest. As a result, the covariance structure is biased downwards and statistical significance therefore reported too high. The calendar time approach, on the other hand, reflects the cross-sectional correlations of individual firms in the portfolio variance and hence provides superior statistical properties.

the changes in the CDS market index ($\Delta r_{m,t}$), similar to the regression design described in equation 4:

$$\Delta r_{p,t} \;=\; \alpha_p + \beta_i(\Delta r_{m,t}) + \epsilon_i. \tag{6}$$

The regression intercept, α_p, can therefore be interpreted as the average daily abnormal spread change of the portfolio consisting of event firms only. Provided the market model is specified well, α_p is equal to zero under the null hypothesis. For my full sample of event firms, I find that α_p is significantly different from zero at the one percent level, with a daily average abnormal spread change of 0.23 percent. This finding provides evidence in favor of a persistent impact of a merger transaction on a firm's default risk. I therefore reject the concern that the observed abnormal change in CDS spreads is only a short term deviation.

6.9 Empty Creditors

The more widely spread use of credit default swaps has raised the issue of "empty creditors". Empty creditors are, according to Bolton and Oehmke (2011), debtholders that have obtained insurance against default, but otherwise retain control rights in and outside bankruptcy. As a result thereof, the incentives of debtholders to keep the risk of a firm at an essentially equal level may shift and lead to inefficiently high numbers of bankruptcies.

The distribution of changes in wealth as discussed at the outset could certainly prove to be attractive enough for creditors to buy insurance and profit in case a firm's default risk increases. Based on the gross nominal amount of CDS outstanding one week prior to the deal announcement, on average USD 13.3bn, the event led to an average increase in the value of CDS outstanding of USD 656m. That is, investors holding CDS contracts prior to the announcement will pay too little for the risk they will carry going forward and hence gain considerably from the merger announcements. Even after netting counterparty transactions, the monetary impact of the change in risk is still a gain of USD 65.1m and thus a manyfold of the average negative effect of USD 17.3m for acquiring firms' bondholders.

In terms of mergers, empty creditors may matter in how a deal is structured. That is, acquiring firms with an overinsured bond portfolio have the incentives of bond and shareholders aligned and hence should be financing deals predominantly with additional debt. As a result, these firms are predicted to increase the default risk significantly. Because there is no data linking an investor's holding of bonds and CDS in the same company, the level of an investor's protection against default risk can only be measured on an aggregate market level. I proxy overinsurance in two ways; first, by considering the total net notional amount of CDS outstanding one week prior to announcement and second, by forming an insurance ratio, calculated as the outstanding net notional amount of CDS for the acquirer one week prior to announcement relative to the most recent annual number of total debt. For both measures, higher values increase the likelihood of overinsurance and hence the prospect of a CDS spread increase as a result of the merger transaction.

Figure 5 ABOUT HERE

I find two pieces of evidence that speak in favor of the overinsurance hypothesis. First, panels (a) and (b) in Figure 5 display the reaction of the abnormal spread change around the date of announcement. While all acquirers show a very close comovement prior and after the event window, firms that have an above median level of net notional CDS outstanding or that protected an above median percentage of their debt, exhibit a substantially larger increase in their abnormal CDS spreads.

Second, comparing the CASC statistically across median splits, I find that firms with above median net notional CDS outstanding, on average, have statistically significant CASCs of 7.06 percent which is more than five-fold of the increase in below median firms (c.f. panel C.1 in Table 4). Similarly, firms with above median protection ratios have CASCs of 5.41 percent, more than double of what is observed for below median firms (c.f. panel C.2 in Table 4).

Even though these two pieces of evidence seem promising, the small size of the sample only allows a limited degree of generalization of this finding. Further prove should be provided in a multivariate framework. To do so, however, it is important to solve the endogeneity problem which arises between the level of insurance chosen and many of the explanatory variables used to describe a merger deal. This goes beyond the scope of this project and will be considered in a follow-up paper.

7 Conclusion

Mergers have been under scrutiny by researchers for a long time. While most research has focused on shareholder value and agency problems related to this question, little has been done to analyze the situation of debt holders during a merger transaction. This paper is targeted to diminish this gap and bring the literature on bondholder value a step ahead. It does so by providing an analysis on the following three questions. First, does a merger transaction impact the default risk of current bondholders? Second, what are the value implications of mergers on bondholders? Third, can the value of nominal CDS outstanding lead to a distortion of bondholder incentives comparable to what the literature on empty creditors suggest?

By measuring abnormal changes in credit default swap spreads around merger announcements, I am able to circumvent the biggest challenges of the previous literature and provide insights not only on the overall impact on default risk, but also on the USD value that has been created or destroyed as a result of the merger.

Merger transactions lead to an increase in the acquirer's debt default risk in two thirds of the deals. The average cumulated abnormal spread change in the three day window around the announcement is 4.95 percent. Based on a basic asset pricing framework, different explanations for why default risk increases are derived. For example, the abnormal increase in CDS spreads is particularly large for mergers that are done within the same

industry, where cash flow diversification possibilities are small. Relative to transactions across industries, which can profit from this co-insurance effect, these deals increase abnormal CDS spreads by a three-fold.

Moreover, the risk does not seem to be driven by the characteristics a target contributes to the merged company, but rather by how a deal is structured. For example, it is not the fact that the target carries a higher leverage ratio than the acquirer that drives the increase in CDS spreads, but rather the circumstance that the deal leads to an increase in the acquirer's level of leverage.

The close relation of CDS spreads and bond prices offers the possibility to not only consider changes in default risk, but also to put a price tag on it. Interestingly, the impact of mergers on bondholder wealth is rather moderate, with an average loss of USD 17.3m. What is striking, however, is the comparable change in wealth of current CDS investors. On average, outstanding CDS contracts gain USD 656m in value as a result of the merger announcement. The considerable difference between the profits for holders of CDS contracts and the losses for bondholders points towards a potentially substantial agency conflict arising in the structuring of the deal.

This paper links the default risk of bonds to changes in their value and provides a first assessment on how mergers effect these channels. Further work needs to be done, especially on the last issue of agency conflicts in deal structuring. With the number of deals in the sample increasing constantly, future research will be able to answer this question in more detail and scope.

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A Asset volatility calculation

I follow Vassalou and Xing (2004) in calculating the asset volatility of the firms involved in mergers considered. Based on the model of Merton (1974), a firm's equity can be thought of as a call option on the firm's total assets, with a strike price equal to the book value of total debt outstanding. The option has a positive value to shareholders in case total assets are larger than total debt and none otherwise. Using the framework of Black and Scholes (1973), the call option can be priced similar to the put option in equation 3, by using the following formula:

$$V_E = V_A N(d_1) - X e^{-rt} N(d_2) \tag{7}$$

with

$$d_1 = \frac{ln(\frac{V_A}{X}) + (r + \frac{1}{2}\sigma_A^2)\tau}{\sigma_A\sqrt{\tau}}, \quad d_2 = d_1 - \sigma_A\sqrt{\tau}$$

Asset volatility (σ_A) is calculated implicitly, using equation 7 in an iterative process. In the first step, I use daily market values of a firm's equity over the past 100 days to estimate σ_E. Using this estimate as a first proxy for σ_A, I calculate the implicit daily market value of assets by solving equation 7 for V_A. In the second step, I use the previous estimates of V_A for the past 100 trading days to update my estimate of σ_A, i.e. I replace the initial value for σ_A (which was σ_E). The updated asset volatility will then be used again in equation 7 for a next round, to solve and update the values of V_A, and so forth. Iterations are done until the difference between two consecutive estimates of σ_A is sufficiently small.

The model is specified as follows. X is the most recent available estimate of book debt as it is defined in Section 5.1. V_E is calculated, with data from CRSP, as shares outstanding multiplied by the daily share price. To have consistency in the length of the contract to the one from CDS spreads, τ is set equal to five years. Finally, r is the treasury rate of a five year constant maturity bill.

Figure 1. Asset Swap Mechanism

This figure presents the mechanism behind the asset swap CDS pricing model. It requires three steps: First, the protection buyer takes up a risk-free bank loan with nominal N_i at Libor r_l. Second, the nominal of the loan is invested in a risky bond with a fixed coupon r_i that is bought at par. The third step involves a hedge against changes in the interest rate level. The protection buyer hence engages in a swap, paying r_i and receiving r_{float}. Finally, the price paid for insurance against default, the CDS price, is the difference between r_{float} and r_l.

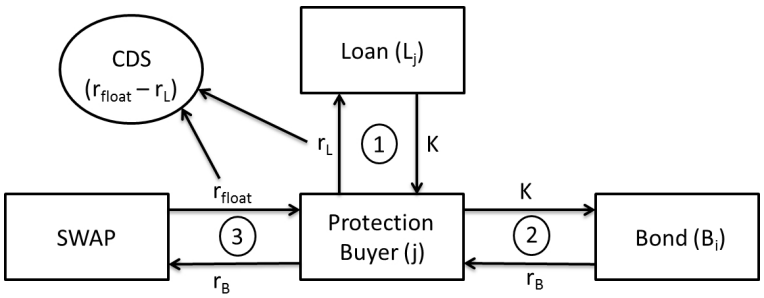


Figure 2. Average abnormal CDS spread change around deal announcements

This figure shows the daily level of cumulative abnormal spreads for the portfolio of acquirers (solid) and targets (dotted) during the 20 day window [-10,+10] around the event. Cumulation of the abnormal spreads starts at t=-10. The portfolios are equal-weighted. The vertical axis represents the daily level of the cumulative abnormal spread while the horizontal axis is measured in days relative to the event (t=0). Abnormal spread changes are calculated with the market model. For acquirers, the daily abnormal spread changes in the event window are +0.41% (t=-1), +2.42% (t=0) and +2.12% (t=1). For targets, the effects were +0.15% (t=-1), -10.10% (t=0) and -9.36% (t=1).

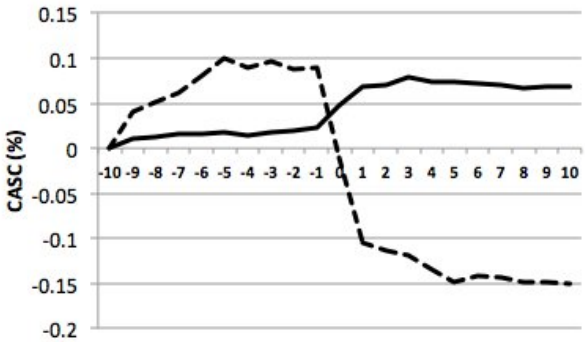
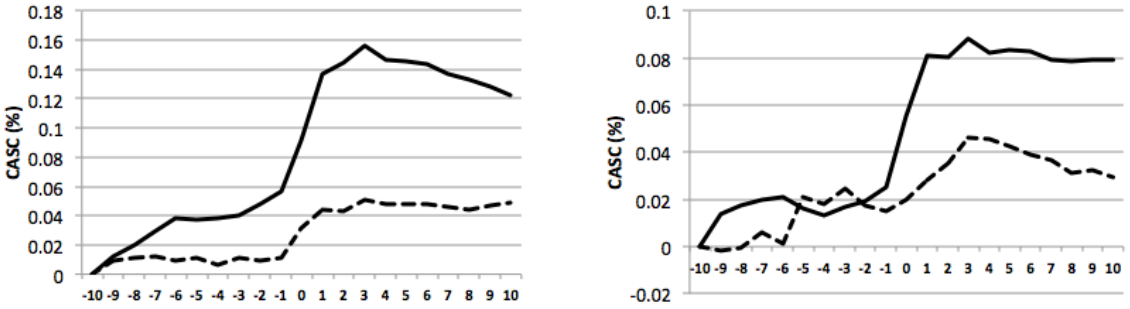


Figure 3. Trends in CASCs of subsamples on deal characteristics

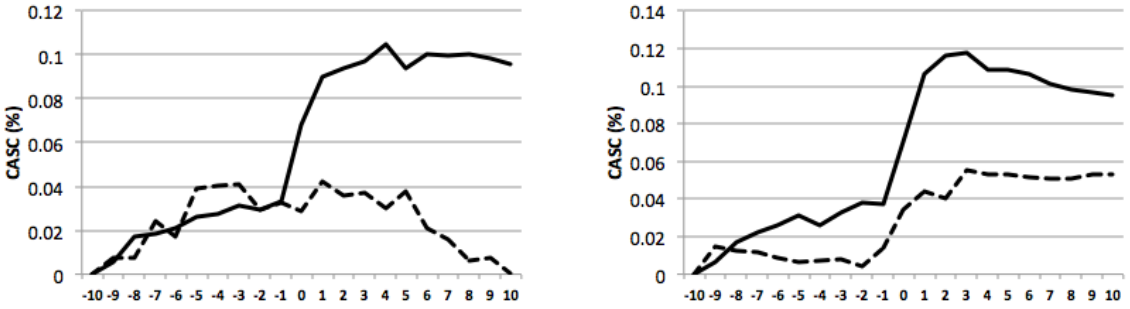
Panels (a) and (b) show the daily level of the cumulative abnormal spread for selected sample splits of acquiring firms during the 20 day window [-10,+10] around the event. Cumulation of the abnormal returns starts at t=-10. The vertical axis represents the daily level of the cumulative abnormal spread while the horizontal axis is measured in days relative to the event (t=0). Abnormal spread changes are calculated with the market model. *Panel (a)* shows mergers within (solid) and across industries (dotted). *Panel (b)* splits along transactions that increase (solid) or decrease (dotted) the acquirer's leverage.



(a) Coinsurance (b) ΔLeverage

Figure 4. Trends in CASCs of subsamples on relative ex-ante firm characteristics

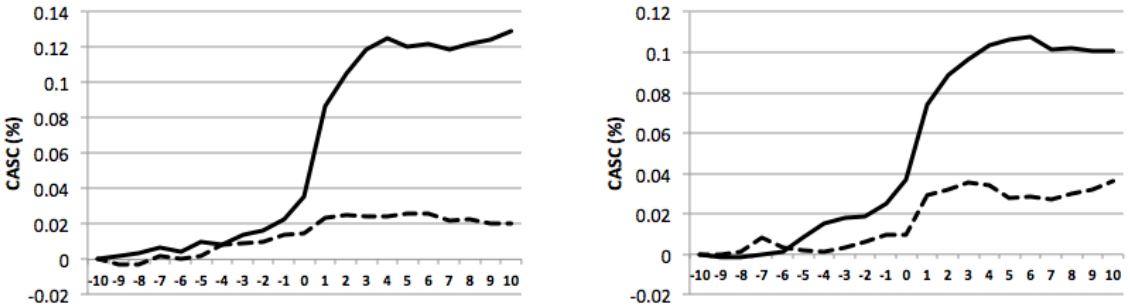
Panels (a) and (b) show the daily level of the cumulative abnormal spread for selected sample splits of acquiring firms during the 20 day window [-10,+10] around the event (t=0). Cumulation of the abnormal returns starts at t=-10. The vertical axis represents the daily level of the cumulative abnormal spread while the horizontal axis is measured in days relative to the event (t=0). Abnormal spread changes are calculated with the market model. *Panel (a)* splits the sample in transactions in which the target has a higher asset volatility than the acquirer ex ante (solid) and vice-versa (dotted). *Panel (b)* splits the sample in transactions in which the target has a higher leverage than the acquirer ex ante (solid) and vice-versa (dotted).



(a) Relative Asset Volatility (b) Relative Leverage

Figure 5. Trends in CASCs of subsamples on levels of CDS protection

Panels (a) and (b) show the daily level of the cumulative abnormal spread for selected sample splits of acquiring firms during the 20 day window [-10,+10] around the event. Cumulation of the abnormal returns starts at t=-10. The vertical axis represents the daily level of the cumulative abnormal spread while the horizontal axis is measured in days relative to the event (t=0). Abnormal spread changes are calculated with the market model. *Panel (a)* splits the sample in deals where the acquirer has an amount of notional CDS outstanding that is above (solid) or below (dotted) the median. *Panel (b)* splits the sample in deals where the acquirer has a share of debt protected by CDS that is above (solid) or below (dotted) the median.



(a) CDS Net Notional

(b) CDS Protection Ratio

Table 1. Summary statistics on credit default swaps for the main sample

This table displays summary statistics on the CDS data obtained from the Depository Trust & Clearing Corporation. *CDS Net National* is the notional amount of CDS outstanding, netted for counterparty contracts. *CDS Net National change* is the change in net notional amount of CDS outstanding in the week of the transaction. *CDS Gross National* is the notional amount of CDS outstanding. *CDS Gross National change* is the change in gross notional amount of CDS outstanding in the week of the transaction. *CDS Net Protection Level* is the ratio of CDS Net Notional relative to a firm's total debt prior to the deal announcement. *CDS Gross Protection Level* is the ratio of CDS Gross Notional relative to a firm's total debt prior to the deal announcement. *CDS Contracts* outstanding is the number of CDS contracts outstanding for a particular firm. *CDS Contract change* is the change in CDS contracts outstanding in the week of the transaction. All change variables have to be interpreted as a lower bound as DTCC only reports levels, but not observed changes. The changes reported here are therefore the residuals left after netting out total market activity.

Variable	Mean	(Std. Dev.)	Min.	Max.	N
CDS Net Notional (bn)	1.19	0.95	0.32	5.95	59
CDS Net Notional change	24.20	38.80	0.01	280	59
CDS Gross Notional (bn)	13.30	8.38	2.52	35.30	59
CDS Gross Notional change	17.90	41.70	0.52	235.00	55
CDS Net Protection Level (%)	18.09	21.60	2.30	108.78	59
CDS Gross Protection Level (%)	231.15	325.98	25.34	1'715.28	59
CDS Contracts outstanding	2'114	1'206	326	5'480	59
CDS Contracts changes	37.88	75.57	0	388	59

Table 2. Summary statistics for the main sample

This table displays summary statistics for acquirer, target and deal specific characteristics. *CASC* is the cumulative abnormal spread change of the credit default swap in the three day window [-1,+1] around the announcement (t=0). CASC is derived with the market model based on the CDX North America IG index. *CDS Spread* is the average observed CDS spread in the market during the three day event window. *Leverage* is defined as the book value of debt relative to total assets (the sum of debt and market value of equity). It is calculated as of end-of-year for the year prior (pre) and the year of the announcement (post). *Put Covenant Ratio* is the share of total debt that is covered by a put covenant. *Debt Time to Maturity* is the average time to maturity of the value-weighted debt portfolio of the firm. *Asset Volatility* is obtained through own calculations. *Asset Beta* is a value-weighted average of the calculated equity beta for the period prior to the announcement [-120,-20] and the assumed debt beta of 0.4. *CDS Net National* is the notional amount of CDS outstanding, netted for counterparty contracts. *CDS Gross National* is the notional amount of CDS outstanding. *Protection Level* is the ratio of CDS Net Notional relative to a firm's total debt prior to the deal announcement. *CDS Contracts* outstanding is the number of CDS contracts outstanding for a particular firm. *CDS Contract* changes is the absolute change, and as such a lower bound, of CDS contracts in the week prior to the announcement. *CIC Covenant Ratio* is the share of total debt that is covered by a change-in-control covenant. *Total Synergies* are defined as the sum of the acquirer and target synergies, valued in millions of USD. They are calculated by multiplying the cumulative abnormal stock return for an eleven day window [-5,+5] around the announcement with the market value of equity six days prior to announcement. *Cash Deal* is a dummy variable equal to one in case the deal is financed with cash only. *Stock Deal* is a dummy variable equal to one in case the deal is financed with stock only. *Cross-Industry* is an indicator variable that compares the four-digit industry SIC code of the target and the acquirer. It is equal to one in case the codes for the target and the acquirer are different. *Deal Ratio* is calculated as the transaction value of the deal (based on the target's enterprise value) relative to the acquirer's pre-runup equity value four weeks prior to the announcement. *Share Acquired* is defined as the share of total target equity acquired in the transaction.

Variable	**Mean**	**(Std. Dev.)**	**Min.**	**Max.**	**N**
Acquiror					
CASC (%)	4.15	13.49	-18.55	79.40	184
CDS Spread (t=0)	113.28	162.71	7.23	1394.72	184
Market Value (m)	53'700	58'300	140	341'000	184
Leverage pre (%)	26.11	20.42	2.20	91.42	184
Leverage post (%)	27.42	21.29	1.82	92.37	155
Put Covenant Ratio (%)	12.41	20.00	0.00	100.00	171
Debt Time to Maturity (y)	8.90	5.30	0.16	40.63	171
Asset Volatility (%)	16.35	9.57	4.72	71.83	153
Asset Beta	0.81	0.24	0.33	1.99	184
Target					
CASC (%)	-18.51	25.21	-91.47	17.85	25
CDS Spread (t=0)	163.34	183.53	22.92	760.37	25
Market Value (m)	5'635	9'397	2.00	59'400	183
Leverage pre (%)	23.17	25.34	0.00	99.69	174
CIC Covenant Ratio (%)	53.86	45.76	0.00	100.00	83
Debt Time to Maturity (y)	9.75	6.76	0.20	33.00	83
Asset Volatility (%)	21.85	14.61	3.41	63.69	53
Asset Beta	0.99	0.44	0.33	2.20	153
Deal					
Total Synergies (m)	383	6'375	-25'200	62'900	154
Cash Deal (%)	41.30	49.37	0.00	100.00	184
Stock Deal (%)	14.81	35.69	0.00	100.00	108
Cross-Industry (%)	75.00	43.42	0.00	100.00	184
Deal Ratio (%)	27.07	43.54	1.03	291.53	184
Share Acquired (%)	96.63	12.25	36.30	100.00	184

Table 3. Correlations of explanatory variables

This table displays pairwise correlations of the main explanatory variables. Variables are defined in Table 2. Acq. is short for acquiring firm and Targ. for target firm. Lev. pre is short for leverage prior to announcement. Put Cov. is equivalent to the variable Put Covenant Ratio and CIC Cov. for Put Covenant Ratio. Maturity stands for the variable Debt Time to Maturity and Vol. is short for Volatility. CDS Net is short for the variable CDS Net Notional. Consideration is a dummy variable equal to one if the consideration offered is cash-only and zero in case consideration is stock-only.

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Acq. CASC	1															
Acq. Lev. pre	-0.034	1														
Put Cov.	-0.053	-0.255	1													
Acq. Maturity	0.084	-0.096	0.215	1												
Acq. Asset Vol.	0.070	0.173	0.029	0.018	1											
Acq. CDS Net	0.155	0.221	-0.190	-0.186	0.026	1										
Targ. CASC	-0.050	0.086	0.030	0.372	-0.113	-0.201	1									
Targ. Lev. pre	0.040	0.514	-0.146	-0.122	0.295	0.138	-0.142	1								
Targ. CIC Cov.	-0.093	-0.011	0.246	0.052	-0.056	-0.200	0.201	-0.233	1							
Targ. Maturity	-0.002	0.003	-0.136	0.134	-0.124	-0.044	0.018	-0.116	-0.217	1						
Targ. Asset Vol.	-0.056	-0.080	0.273	0.194	0.498	-0.288	0.021	-0.168	0.449	0.171	1					
Total Synergies	0.142	0.079	0.007	0.030	0.254	0.210	0.128	0.092	-0.102	-0.060	0.009	1				
Cross-Industry	-0.186	-0.131	0.019	0.003	-0.089	0.138	-0.084	-0.164	0.171	-0.117	0.118	-0.156	1			
Consideration	0.108	-0.404	0.132	-0.037	-0.236	0.143	-0.405	-0.445	0.471	0.106	0.440	-0.209	0.333	1		
Deal Ratio	0.229	0.248	0.001	0.007	0.038	0.152	0.059	0.267	-0.200	0.045	-0.223	0.036	-0.230	-0.547	1	
Share Acquired	-0.002	-0.104	0.088	0.035	-0.043	-0.093	-0.209	-0.077	-0.054	-0.068	-0.201	0.006	0.042	0.053	0.108	1

Table 4. Explaining abnormal CDS spread changes: univariate splits

This table displays cumulative abnormal spread changes (CASC) during the three day event window [-1,1] in selected sample splits according to variables of interest described in Table 2. If not indicated differently, the variables are based on the acquiring firms only. *Panel A* centers around differences in deal characteristics, *Panel B* looks at relative differences between acquirers and targets prior to the deal and *Panel C* presents sample splits along proxy variables for empty creditors. Panel A.7 is based on data from targets only. The t-statistics are calculated by forming equal weighted portfolios according the specified sample splits. Based on the portfolio's daily abnormal spread change, standard deviations are calculated over a 100 day window prior to the run-up period [-120,20].

Panel A.1: Deal Ratio

	CASC (%)	t-value	n
Below Median	0.69	1.18	92
Above Median	8.32	8.83	92
Difference	-7.63	-9.98	

Panel A.2: Cross-Industry

	CASC (%)	t-value	n
Same SIC	8.91	5.88	46
Different SIC	3.49	6.51	138
Difference	5.42	6.94	

Panel A.3: Total Synergies

	CASC (%)	t-value	n
Negative	3.14	4.79	78
Positive	6.29	7.25	76
Difference	3.15	4.15	

Panel A.4: ΔLeverage

	CASC (%)	t-value	n
Increase	6.14	8.73	74
Decrease	1.08	1.29	39
Difference	-5.07	-6.78	

Panel A.5: Book Debt

	CASC (%)	t-value	n
Increase	5.53	10.96	79
Decrease	2.25	0.95	33
Difference	-3.28	-3.10	

Panel A.6: Consideration

	CASC	t-value	n
Cash Deals	3.93	5.53	76
Stock Deals	-2.24	-0.51	16
Difference	6.17	4.59	

Panel A.7: CIC Covenant Ratio

	CASC (%)	t-value	n
Below 50%	8.34	4.61	41
Above 50%	5.69	6.05	42
Difference	-2.65	-1.94	

Panel A.8: Put Covenant Ratio

	CASC (%)	t-value	n
Below 50%	6.22	10.07	158
Above 50%	-1.12	-0.64	13
Difference	-7.33	-10.43	

Panel B.1: Asset Beta

	CASC (%)	t-value	n
Acquirer > Target	8.61	9.46	59
Acquirer < Target	3.19	3.58	94
Difference	-5.41	-6.02	

Panel B.2: Asset Volatility

	CASC (%)	t-value	n
Acquirer > Target	1.29	0.76	14
Acquirer < Target	6.07	7.15	39
Difference	4.78	4.46	

Panel B.3: Relative Leverage

	CASC (%)	t-value	n
Acquirer > Target	3.98	6.01	98
Acquirer < Target	6.84	5.89	76
Difference	-2.86	-3.26	

Panel B.4: Time to Maturity

	CASC (%)	t-value	n
Acquirer > Target	6.63	4.05	50
Acquirer < Target	7.97	6.77	32
Difference	-1.34	-0.92	

Panel C.1: CDS Net Notional

	CASC (%)	t-value	n
Below Median	1.36	2.04	30
Above Median	7.06	8.28	29
	-5.70	-7.51	

Panel C.2: CDS Protection Level

	CASC (%)	t-value	n
Below Median	2.56	3.24	30
Above Median	5.41	8.00	29
	-2.85	-3.87	

Table 5. Explaining abnormal CDS spread changes: deal characteristics I

Regressions in this table are based on the full sample. The limited availability of some data may lead to a reduction in sample size in some settings. The dependent variable in these regressions is the log of the acquirer's cumulative abnormal spread changes (CASC) over the three day window around the merger announcement [-1,1]. The explanatory variables are defined in Table 2. Δ*Leverage* is the change in percentage points of the acquirer's leverage during the year of the announcement. *Consideration* is a dummy variable equal to one if the consideration offered is cash-only and zero in case consideration is stock-only. *After Big Bang* is a dummy variable equal to one for deals that were announced after the introduction of the Big Bang agreement on April 8th 2009. t-values are calculated based on robust standard errors and reported in brackets, with significance levels: * 0.10, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cross-Industry		-0.042*					
		(-1.76)					
Total Synergies			0.008**				
			(2.28)				
ΔLeverage				0.003*			
				(1.76)			
Consideration					0.096**		
					(2.23)		
CIC Covenant						-0.016	
						(-0.50)	
Put Covenant							-0.042*
							(-1.70)
Deal Ratio	0.044*	0.035*	0.046*	0.046*	0.180*	0.032	0.047*
	(1.90)	(1.70)	(1.75)	(1.71)	(1.84)	(1.26)	(1.90)
Acquirer Asset Volatility	0.081	0.068	0.079	0.079	0.033	0.095	0.105
	(0.83)	(0.72)	(0.66)	(0.85)	(0.36)	(0.75)	(0.96)
After Big Bang	-0.003	-0.007	-0.002	0.015	-0.002	0.001	0.000
	(-0.19)	(-0.43)	(-0.13)	(0.91)	(-0.14)	(0.04)	(0.03)
Constant	0.008	0.045*	0.010	-0.005	-0.093*	0.028	0.009
	(0.39)	(1.66)	(0.49)	(-0.27)	(-1.91)	(0.80)	(0.46)
Observations	153	153	127	148	75	72	143
Adjusted R-squared	0.026	0.051	0.085	0.074	0.141	-0.028	0.038

Table 6. Explaining abnormal CDS spread changes: deal characteristics II

Regressions in this table are based on the full sample. The limited availability of some data may lead to a reduction in sample size in some settings. The dependent variable in these regressions is the log of the acquirer's cumulative abnormal spread changes (CASC) over the three day window around the merger announcement [-1,1]. The explanatory variables are defined in Table 2. Δ*Leverage* is the change in percentage points of the acquirer's leverage during the year of the announcement. *Consideration* is a dummy variable equal to one if the consideration offered is cash-only and zero in case consideration is stock-only. *After Big Bang* is a dummy variable equal to one for deals that were announced after the introduction of the Big Bang agreement on April 8th 2009. t-values are calculated based on robust standard errors and reported in brackets, with significance levels: * 0.10, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)
Cross-Industry	-0.056**	-0.049*	-0.046	-0.086*	-0.058**
	(-2.02)	(-1.84)	(-1.36)	(-1.80)	(-2.10)
Total Synergies	0.008**	0.007**	0.001	0.011**	0.008**
	(2.25)	(2.25)	(0.45)	(2.07)	(2.41)
ΔLeverage		0.003*	0.004**	0.003	0.002
		(1.73)	(2.07)	(1.39)	(1.53)
Consideration			0.048		
			(1.45)		
CIC Covenant				0.042	
				(1.12)	
Put Covenant					-0.053*
					(-1.92)
Deal Ratio	0.038*	0.038	0.078	0.043	0.046
	(1.68)	(1.54)	(0.84)	(1.47)	(1.65)
Acquirer Asset Volatility	0.040	0.039	-0.130	0.026	0.050
	(0.34)	(0.34)	(-1.07)	(0.14)	(0.37)
After Big Bang	-0.005	0.012	0.016	0.025	0.013
	(-0.27)	(0.58)	(1.34)	(0.60)	(0.65)
Constant	0.062*	0.044	0.002	0.050	0.057*
	(1.94)	(1.46)	(0.03)	(1.18)	(1.71)
Observations	127	126	66	64	117
Adjusted R-squared	0.125	0.163	0.321	0.171	0.201

Table 7. Explaining abnormal CDS spread changes: relative characteristics

Regressions in this table are based on the full sample. The limited availability of some data may lead to a reduction in sample size in some settings. The dependent variable in these regressions is the log of the acquirer's cumulative abnormal spread changes (CASC) over the three day window around the merger announcement [-1,1]. The explanatory variables are defined in Table 2. *Higher Target Leverage* is a dummy variable equal to one if the target's leverage level prior to the deal announcement is higher than the acquirer's and 0 otherwise. *Longer Target Maturity* is a dummy variable equal to one if the target's debt time to maturity is longer than the acquirer's and 0 otherwise. *Higher Target Asset Volatility* is a dummy variable equal to one if the target's asset volatility prior to the deal announcement is higher than the acquirer's and 0 otherwise. *Higher Target Asset Beta* is a dummy variable equal to one if the target's asset beta prior to the deal announcement is higher than the acquirer's and 0 otherwise. Δ*Leverage* is the change in percentage points of the acquirer's leverage during the year of the announcement. *After Big Bang* is a dummy variable equal to one for deals that were announced after the introduction of the Big Bang agreement on April 8th 2009. t-values are calculated based on robust standard errors and reported in brackets, with significance levels: * 0.10, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Higher Target Leverage	0.016				0.007			
	(0.87)				(0.33)			
Longer Target Duration		0.013				-0.002		
		(0.39)				(-0.05)		
Higher Target Asset Beta			-0.024				-0.029	
			(-1.17)				(-1.50)	
Higher Target Asset Volatility				0.038				0.035
				(1.48)				(1.20)
Cross-Industry					-0.047*	-0.063	-0.046*	-0.033
					(-1.66)	(-1.37)	(-1.76)	(-1.00)
Total Synergies					0.008**	0.011**	0.008**	0.005
					(2.28)	(2.21)	(2.36)	(1.21)
ΔLeverage					0.003*	0.001	0.003*	0.000
					(1.76)	(0.61)	(1.82)	(0.15)
Deal Ratio	0.046*	0.037	0.045*	0.016	0.036	0.040	0.033	0.019
	(1.70)	(1.38)	(1.67)	(0.77)	(1.44)	(1.39)	(1.45)	(0.85)
Acquirer Asset Volatility	0.052	0.111	0.087	-0.048	0.022	0.009	0.013	-0.009
	(0.55)	(0.85)	(0.70)	(-0.36)	(0.18)	(0.05)	(0.12)	(-0.06)
After Big Bang	-0.002	0.011	-0.005	0.006	0.014	0.025	0.016	0.004
	(-0.10)	(0.36)	(-0.25)	(0.22)	(0.65)	(0.60)	(0.78)	(0.12)
Constant	0.006	0.001	0.026	0.002	0.042	0.059	0.064*	0.026
	(0.28)	(0.02)	(1.07)	(0.07)	(1.32)	(0.94)	(1.80)	(0.75)
Observations	145	70	126	53	125	62	125	50
Adjusted R-squared	0.028	-0.019	0.030	-0.034	0.158	0.126	0.174	-0.034

Part III: Curriculum Vitae

Christoph Thomas Wenk Bernasconi — April 2013

Date of Birth	5th October 1981
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Research	Corporate Governance, Mergers & Acquisitions, Two-sided markets, Peer-to-peer lending
Teaching	Corporate Finance, Valuation, Mergers & Acquisitions, Empirical Corporate Finance

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University of Zurich, Switzerland

PhD Program in Finance — **January 2009 – April 2013**

- Advisors: Professor Dr. Alexander F. Wagner, Professor Dr. Kjell G. Nyborg
- Title: Essays in Empirical Corporate Finance
- Grade: Summa Cum Laude
- Advanced Coursework:
  The Econometrics of Auctions, Prof. Robert Porter, 2010
  Empirical Strategies, Prof. Joshua Angrist, 2009
  Empirical Corporate Finance, Prof. Professor Alexander Ljungqvist, 2009

Study Center Gerzensee, Switzerland

Swiss Program for Beginning Doctoral Students — **January 2008 – February 2009**

- Econometrics
  Proff. Bo Honoré and Mark Watson
- Microeconomics
  Proff. M. Dewatripont, J. Moore, J-C. Rochet, K. Schmidt
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Master of Arts in Business Administration — **August 2005 – October 2007**

- Major: Financial Economics
- Master Thesis: The determinants of lending - Empirical Evidence from Prosper.com
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- Bachelor Thesis: An efficient investment portfolio for the Swiss electricity supply sector
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